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"THE HISTORIAN LOOKS AT MEDICINE" *

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It is a nice question, so far as research is concerned, whether there is such a subject as "history." One cannot delve into history *per se*, but only into the history of something—of government, of economics, or of the arts and sciences. There is an historical aspect of, or historical approach to all these subjects; and research historians might be logically defined as so many economists, architects, or physicians who study their respective subjects in the time perspective. But all this is a matter of logical distinctions, and human institutions are notoriously illogical in their development. What actually evolved in American universities during recent generations, was an able professional group which was concerned primarily with the historical study of political and economic conditions. They came to be known as *the* historians, and their interests were held to circumscribe a field known as general History—history with a capital "H." This left those concerned primarily with the historical study of other subjects—social institutions, art, science—without the professional pale and restricted to their immediate fields.[1]

*A paper read at the annual meeting of the American Association of Medical History at Atlantic City, N. J., May 3, 1937.

[1] Certain exceptions to these statements should be noted; e. g. general historians writing on the Renaissance period were always much interested in the fine arts. But there was an obvious tendency, after 1860, to stress political, military, and (somewhat later) economic history.

Such differentiation was useful, but also had its limitations. The general historians held full-time chairs, and could often give more time to the study of historical materials and processes than could those devoted to special fields. In a sense, the former became professionals, while the latter remained amateurs. Perhaps in consequence the general historians were able to emphasize continuity and literary form in their narratives to an unusual degree. They were also likely to base their work upon a wider range of sources. They exploited newspapers, correspondence, and governmental publications. Medical historians, in contrast, were apt to limit themselves to the study of published technical materials. They were naturally concerned with the writings of prominent physicians; but sometimes overlooked the information conveyed by informal correspondence or by impersonal documents and public prints.

Meanwhile, the general historians felt that they were constructing History in the only sense in which it may be said to constitute a subject in itself; that is, as a written synthesis of all the most important happenings in the past. They worked out—or just took for granted—a philosophy which indicated what was significant for this purpose. It appeared that only those fields in which general historians wished to do research, were also worthy of inclusion in the grand synthesis. They wrote studies with such sweeping titles as "The History of the United States," in which there was little mention of art, less of law, and almost nothing of science and technology. Despite the degree to which science has made the modern world, this vital subject was either omitted from the story or pushed aside in supplementary paragraphs. So it happened that while historians of the arts and sciences used little else than technical materials, general historians frequently went to the other extreme and omitted technical literature altogether.[2]

Fortunately, this situation has been changing during recent decades. General historians are attempting once more to bring the story of the arts and sciences into their purview under the headings of "social" or "cultural" history.[3] Especial interest seems

[2] Richard H. Shryock, "Medical Sources and the Social Historian," *Amer. Hist. Rev.*, XLI (1936), 459.

[3] Excellent examples will be found, among recent American works, in Preserved

to have developed in this country in medical history—notably among the medievalists, but of late even among those devoted to the American scene. Evidence of this is to be found in various studies relating to medical science or to the medical profession, and also in occasional articles on similar themes now appearing in general historical periodicals.[4]

More than medicine is involved. At least a few scholars are coming to view the development of all the sciences as an essential part of their program. The "old humanists," as Sarton terms them, affected a disdain for science as "the merely practical," but the "new humanists" see in it one of the noblest as well as one of the most significant of human activities. Hence they expect the study of the history of science to enrich historical content and at the same time to humanize the sciences themselves. In a word, it is hoped that such work will bridge the gap between the "old humanism" and the sciences, to their mutual advantage.[5] Some years ago, the Johns Hopkins University declared its readiness to award the doctor's degree in the history of science; and although various difficulties prevented this in practice, the plan was not without significance. Within the last year, Harvard University has declared its intention to grant the degree in this same field.[6]

On the other hand, it is apparent that the historians of science in general and medical historians in particular are becoming conscious of the social and cultural background of their immediate themes. The latter are employing general historical studies, and frequently

Smith, *A History of Modern Culture*, I (N. Y., 1930); in Charles A. and Mary R. Beard, *The Rise of American Civilization* (N. Y., 1930); and in the several volumes of *A History of American Life*, edited by D. R. Fox and A. M. Schlesinger. See especially, A. M. Schlesinger (Ed.), *Historical Scholarship in America* (N. Y., 1932), pp. 14 ff.

[4] The contributions of Charles H. Haskins, James Westfall Thompson, and Lynn Thorndyke are well known. Similar interest in medical themes is to be found in the work of such younger American medievalists as Dorothy Mackay (Mrs. W. R. Quynn) and Loren C. MacKinney. Among the historians making contributions to American medical history in recent years have been St. J. R. Childs, Nathan Goodman, C. R. Hall, Helen A. Marshall, and H. B. Shafer.

[5] George Sarton, *The History of Science and the New Humanism* (N. Y., 1931), pp. 26 ff., 72.

[6] As explained in the last Annual Report of President Conant.

envisage their field as a part of the whole complex of social and cultural evolution. This perspective, on their part, is encouraged by the small group of professional medical historians—physicians who have been enabled, by the establishment of special chairs and institutes, to give much or all of their time to historical studies.

Of obvious importance in this country has been the work of the Institute of the History of Medicine at the Johns Hopkins University, where provision is carefully made for close cooperation between physicians, and medical and general historians. Less systematic but similar cooperation can be observed in the informal contacts of medical men, social scientists, and historians in other institutions. Recent experiments in the study of "American Civilization"—on the undergraduate level at the University of Michigan, and now in the Graduate School at Harvard—also involve the correlation of the work of academic and scientific historians.

All this is not to say that two groups have completely bridged the gap between them. Rather is this work just begun, and the difficulties in continuing it are considerable. The historian who wishes to trace the progress of the sciences will have his troubles. How can he orientate himself in the technical literature of so many disciplines? The historian of science faces an analogous difficulty with relation to innumerable writings on social and cultural development. No one man, to be sure, will be expected to surmount all of these obstacles; but it will be no easy matter to assemble even a cooperative group capable of doing so.

Problems of aptitude and training are also involved. The question has been seriously raised: Who should write the history of medicine, the physician who is an amateur or the physician who is a professional medical historian? And there is the analogous query: Who is best qualified to study the history of science, the scientist or the historian? These may be important questions, in principle; but at this stage of the game, they can be taken too seriously. With so much to be done, it would seem wise to welcome the aid of all physicians and scholars who are sufficiently interested to coöperate with one another in this field. They may be encouraged in most cases to overcome as far as possible whatever limitations are inherent in their particular equipment. Those few whose training is ideal, in

that it approximates a complete orientation, would usually be the last to disdain the cooperation of others.

To be specific, general historians who wish to contribute to the history of medicine, must depend constantly upon the generous assistance of physicians who share their enthusiasms. In return for such aid, historians may proffer certain ideas or procedures growing out of their own experience. These general suggestions concerning the future study of medical history, as a general historian would envisage the subject, may be grouped under the headings of (1) subjects for investigation and (2) the sources which may be employed.

Medical historians are naturally most interested in the medical sciences, and in the biographies of the leaders who developed these sciences. These will remain the most essential subjects. But closely related to them are other themes which may be worthy of more attention than is sometimes accorded them. Most obvious is the history of the medical profession as a social group. Chauncey Leake's work on *Percival's Medical Ethics* shows what interesting possibilities lie here. Academic historians wish to study the medical profession as one of the learned groups which set the cultural tone and afforded leadership to society as a whole. They may well ask, for example, whether our society would not be the better for it, if there were a few more physicians or professors in our legislatures, and not quite so many lawyers.

Another significant theme is the influence exerted by professional conditions upon medical research. A most important topic which has not been investigated as much as it deserves, is the lag between research and practice. How long has it taken, in certain periods and with given types of professional organization, for the discovery of the medical center to reach the periphery of average practice? To the social historian, this is a most vital question; yet we sometimes ignore it by writing as though once a discovery was made, it became immediately the common property of the medical world. We say, for example, that antiseptic procedures were introduced in surgery about 1867, because that was when Lister announced their use; but Sigerist has shown what a long and surprising delay there

was in the adoption of such procedures by even leading surgeons in this country.[7]

This suggestion leads naturally into a plea for further studies in the history of the public health. Here indeed is the field where general historians and medical historians—to say nothing of the social scientists—should meet upon common ground. Yet the indifference of many general historians to so vital an aspect of all social evolution as the public health, is still an astonishing phenomenon. To this day, no one has attempted to write a general history of the public health in the United States. There is nothing comparable, therefore, to Sir John Simon's work on England or to Alfons Fischer's volumes on Germany. Medical men have done more than the general historians, as is witnessed by W. T. Howard's thorough study of the history of Baltimore, and by the interesting cooperative volume on *A Half Century of Public Health* which was edited more than a decade ago by Dr. M. P. Ravenel for the American Public Health Association. Such studies will be most valuable to all future historians of the subject.

One aspect of medical practice which necessarily looms large in the history of public health, is the long story of irregular practice. Sectarianism, quackery, and folk medicine may all—from the sociologist's point of view—be quite as significant as regular medicine. It is hardly necessary to point out that this story is at one and the same time fascinating and discouraging, very human and very bizarre. It merits more serious and consistent attention than is usually accorded it in this country.[8] It may appear upon adequate study, for example, that the story of Christian Science has important implications not only for the history of psychiatry but even for the evolution of pathology and other basic sciences.

So much for subjects—for the phases of medical history and related fields which seem worthy of further emphasis. In conclusion, there are several suggestions which a general historian might

[7] Henry E. Sigerist, "Surgery at the Time of the Introduction of Antisepsis," *Jour. Missouri State Med. Asso.* (May, 1935), pp. 169 ff.

[8] More serious work on the history of folk medicine has been done by European than by American medical historians. Note, e. g., Paul Diepgen's comprehensive *Deutsche Volksmedizin: Wissenschaftliche Heilkunde u. Kultur* (Stuttgart, 1935).

make in relation to the sources to be employed. Research in the history of public health implies, almost by definition, the use of newspapers, popular magazines, legal materials, and government documents to a degree not heretofore common in medical historiography.[9] Much the same may be said of studies in professional history, or in the development of irregular practice. The blatant advertising of abortifacient drugs in the American magazines of 1850, for example, may throw considerable light on that particular form of medical service.

In the history of medical science proper, as has been noted, one must depend primarily upon the writings of physicians and other scientists. But these need not necessarily be limited to books and articles. Greater use might be made of the informal correspondence or journals of physicians. These supplement the formal writings, and just because of their intimate nature may throw a more penetrating light—particularly upon professional as distinct from scientific developments.[10] It might prove interesting to find and publish the informal writings of able American physicians, covering eventually the period from about 1760 to the present time.

When the phrase "able physicians" is employed, it does not necessarily denote only those whose findings were later vindicated and generally accepted. Why not publish some of the papers, formal or informal, of those who were aligned on what proved to be the losing side of professional controversy—of those whose work exerted some influence, albeit a retarding influence, upon medical progress?[11] Without more knowledge than we usually secure of such men, we cannot even understand the leaders who combatted

[9] Dr. S. V. Larkey, of the Welch Medical Library of the Johns Hopkins University, has recently pointed out the value of public document series to medical libraries, and has prepared a tentative list of such documents which should prove very useful to medical historians.

[10] An interesting example has just come to my attention in Hubert A. Royster's address on *The Adventurous Life of Edward Warren Bey*, made before the Medical History Section of the Richmond Academy of Medicine (1937), and based upon thirty-five published letters written by Dr. Warren in 1885.

[11] It has always appeared to me that we make too little use of our mistakes—in medical history as in various other fields. If I recall correctly, the same suggestion about the "villains" of medical history was made some years ago by Dr. Henry E. Sigerist.

them. How can we really appreciate Oliver Wendell Holmes' classic paper on puerperal fever, if we do not also read his opponent, Dr. Meigs? By emphasizing chiefly the bright spots in our scientific past, we oversimplify it and thereby render it unreal.

We may eventually secure more adequate studies of those physicians who blindly opposed the "experimental philosophy" in the seventeenth century, of those who elaborated absurd "systems" in the eighteenth century, and of those who opposed the adoption of aseptic procedures during more recent generations. We should not want too much of these "villains" of medical history, but there is no immediate danger of such an overemphasis. With this plea for our forgotten villainies, the historian had best rest his case.

A NOTE ON FRANCESCO GENNARI AND THE EARLY HISTORY OF CYTOARCHITECTURAL STUDIES OF THE CEREBRAL CORTEX

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" Differentiation of structure means differentiation of function."
HUGHLINGS JACKSON.

Recent revival of interest in cerebral cytoarchitecture makes timely an enquiry into the history of the subject. Few historical details are available in the writings of recent students of cytoarchitecture and accurate bibliographical citations are almost uniformly lacking. All earlier authors who have dealt with the subject have therefore been studied afresh.

Gennari, Vicq d'Azyr and Soemmering

The first investigator to find evidence of regional differentiation in the cerebral cortex was an Italian medical student named Francesco Gennari, born in 1750 at Mataleto de Langhirano, a village near Parma. Nothing is known of his early life, except that he received his medical education at Parma and that while there he had opportunity to study under the distinguished pathologist, Michele Girardi (1731-1797), editor of Santorini (*20*), innoculator, pupil of Morgagni, and one of the most virile minds in Italian medicine at that period. Under Girardi's stimulating guidance Gennari became fired with enthusiasm for the study of morbid anatomy, and while a student of 23 years he made the observation on the structure of the cerebral cortex which gave him his subsequent reputation. He received his degree at Parma in 1776 " having already earned fame as a learned doctor and an acute anatomist." The name of Gennari has not found its way into any of the biographical dictionaries in medicine, and there is no contemporary record of his life. Such details as are available locally in Italy were recorded in 1900 by Gennari's fellow townsman of the Parma medical faculty, Francesco Marimò (*29*, see also de Renzi, *34*). Marimò records

FRANCISCI GENNARI

PARMENSIS

MEDICINAE DOCTORIS COLLEGIATI

DE PECULIARI STRUCTURA CEREBRI

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ACCEDUNT.

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PARMAE

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CUM APPROBATIONE.

Fig. 1.

The title-page of Gennari's book (1782) describing stratification of the cerebral cortex (see Appendix).

that because of Gennari's poverty as a student he was given an annual subsidy and that he used part of the proceeds for the benefit of the indigent, but Marimò adds that Gennari's passion for gambling caused him to be in constant financial difficulty. He died young, but the year of his death is not known; it occurred some time prior to 1797. According to Marimò he left only two works other than his account of the structure of the cortex: one, a case report of a hospital attendant of his acquaintance, and another a "medical" work which Marimò (*29*) states was entirely lost. I have been unable to trace either contribution.

Gennari's book, *De peculiari structura cerebri nonnullisque eius morbis*, which is fully described in the Appendix, appeared at Parma in 1782 (Fig. 1). Here Gennari records that on February 2, 1776, on sectioning the brain of a frozen human cadaver, he observed a white line in the cortical substance of the occipital lobe which he designated "Lineola albidior." The passage runs as follows (*19*, pp. 72-75):

"None of the anatomists I have come upon in reading have taught that, besides the cortical substance and the medulla, there exists in the brain another substance which I shall call the third substance of this viscus. If anyone has looked at the horizontal strata of the cortex of a brain dissected at the point where it conjoins with the medulla, he will have found a certain whitish substance ('subalbida'). This does not always present itself in exactly the same spot, appearing a little farther from or a little nearer to the medulla; it is however always observed with the same extension, with the same color, sometimes more abundant and sometimes more obscure; sometimes subtle, sometimes clearer; at points, difficult, at others it is easy to demonstrate. In a defect of a section, especially if it is transected obliquely, this substance disappears; not only in single brains, but also in diverse parts of the same brain I have observed such variations, as if many times it reappeared in such points, *afterwards with a duplicate line* [Italics ours]. Especially in the anterior parts or solely 'subobscure,' it is possible to retrace it, but since it becomes denser to the posterior, I have come to the conclusion that its appearance can be more certainly obtained. In the internal part of the posterior lobe of the brain not far from the point in which it extends itself into the tentorium was where the substance of which I am speaking stretched in a line even whiter, but I have not observed it in any other place to be so evident (Fig. 2).

"This line I first saw the 2nd February 1776 and having often followed it, it seemed to run finally to the ultimate substance of the hippocampus. For the rest I am ignorant to what purpose that substance may be formed, like so

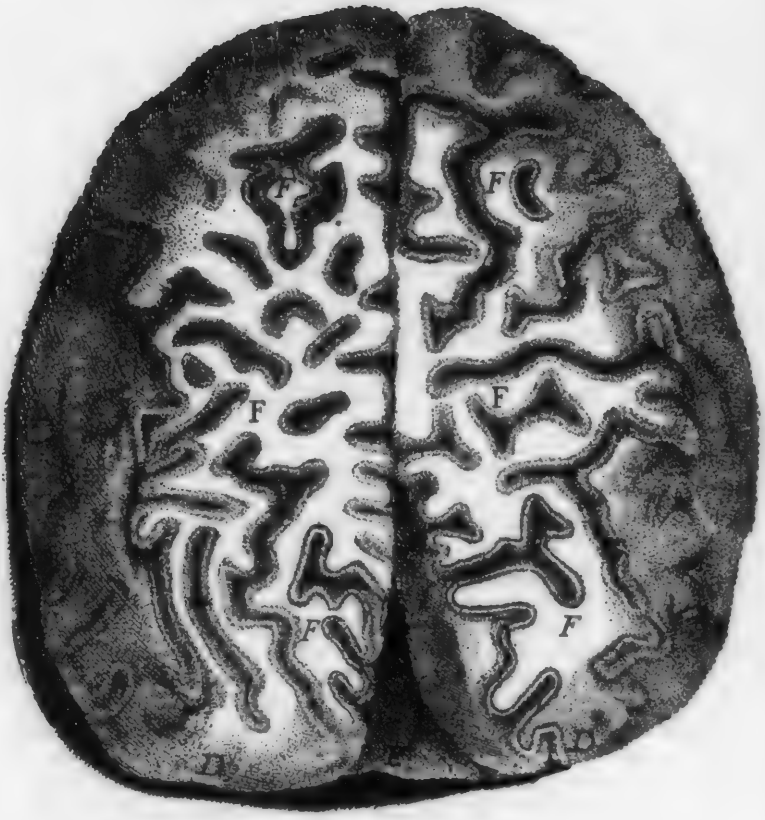

Fig. 2.

Gennari's figure illustrating the whitish line in the occipital cortex (lower portion of plate).

many others of which the use is still hidden, at any rate some of those who combat by the animal spirits, may perhaps draw such an argument for their existence, which in every time, even the hour in which I write, is much disputed. I exhort the anatomists in every way to enquire into this substance."

Gennari's clear recognition of one of the principal structural laminae of the cortex (Fig. 2) was lost sight of, although Baillarger (*1*, 1840) mentions it. It was Obersteiner (*32*) in 1888 who reinvestigated Gennari's work and named the conspicuous calcarine lamina "The line of Gennari."

In addition to describing the line, Gennari's book contains other observations on the nervous system, among them a description of frozen cerebrospinal fluid. Gennari, it appears, was not familiar with Cotugno's (*15*) description of cerebrospinal fluid in 1761; thus Verga notes (*39*): "The Parmesan Gennari in dissecting brains artificially frozen discovered that he always found in their ventricles water converted into minute icicles which in states of disease might be much augmented; water was found also in the intermediate spaces, the periphery of the nervous system, he justly thought that the brain water could not serve otherwise than to diminish the attrition and the compression of the encephalic organ in many of its movements."

Among modern writers the line of Gennari often passes under the name of Vicq d'Azyr (1748-1799), for in 1786, four years after Gennari's book had appeared and ten years after he had first discovered the stria, Vicq d'Azyr (*40*) unaware of Gennari's work, redescribed the white laminae in the calcarine area stating that it was absent from other regions of the brain.[1] The remarkable vol-

[1] Dr. Temkin has directed my attention to the fact that Vicq d'Azyr mentioned the line of white matter in the occipital in a paper entitled: "Recherches sur la structure du cerveau, du cervelet, de la moelle alongée, de la moelle épinière; et sur l'origine des nerfs de l'homme et des animaux" which appeared in *Histoire de l'académie royale des sciences* for 1781 (published in 1784). The statement (p. 511) is as follows:

> "Si, en préparant le centre ovale de Vieussens, on examine la forme des circonvolutions cérébrales postérieures qui sont adossées à la faulx au-dessus du cervelet, on en observe ordinairement plusieurs qui sont remarquables en ce que la substance corticale y est interrompue, suivant sa longueur, par un trait blanc linéaire qui suit tous les contours des circonvolutions, et qui donne à cette portion de la substance corticale, l'apparence d'un ruban rayé: je n'ai point trouvé cette disposition dans les autres régions du cerveau."

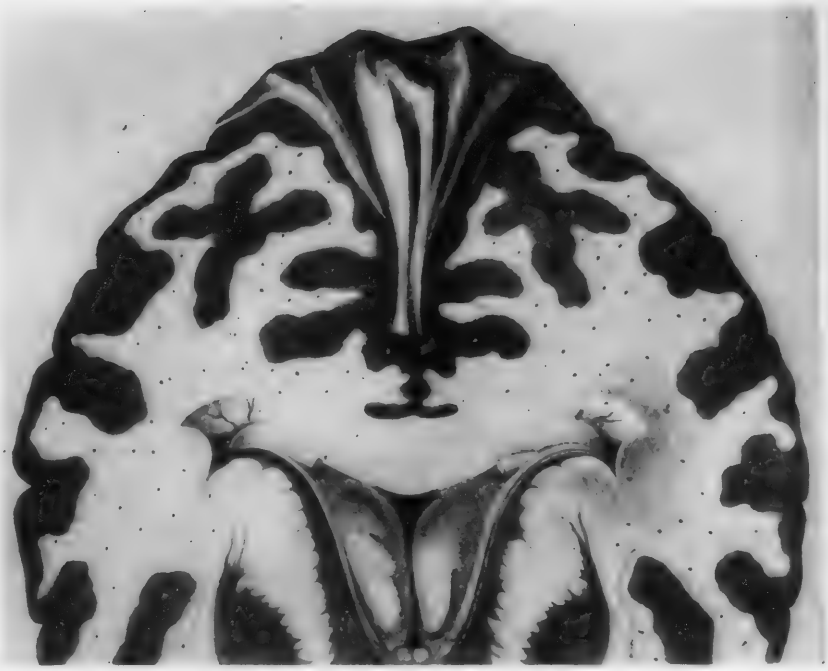

Fig. 3.

Vicq d'Azyr's plate showing the white line as restricted to the calcarine area. The original of this plate which is colored, is in Vicq d'Azyr's monumental folio (*40*, Explanation des Planches, p. 26, paragraph 10, Planche X).

ume in which Vicq d'Azyr's description appears contains some of the finest colored plates of the brain and nervous system to be found in neurological literature prior to the 19th century. Vicq d'Azyr's Plate X showing the occipital cortex is herewith reproduced (Fig. 3). The line of Gennari was also seen and fully described in 1788 (p. 18) by the German anatomist, Samuel Thomas Soemmering (1755-1830; *38*, p. 20) who similarly was unaware of Gennari's work.

Jules Gabriel–François Baillarger

The observations of Gennari, Vicq d'Azyr and Soemmering were made with the naked eye by gross dissection. Baillarger (1806-1891), the Parisian psychiatrist, in 1840 made a considerable advance by cutting thin slices of fresh cortex and studying them also with the naked eye, but held to the light between two pieces of glass. By this means and apparently without the aid of a microscope he divided the cortex into six layers of alternate white and gray lamina (Fig. 4). In his first paper (*1*) he not only recognized these details concerning the stratification of the cortex, but he was also able to satisfy himself that the white line seen by Gennari in the occipital area could be traced in all parts of the cortex, although it was far less conspicuous anteriorly than posteriorly. This continuation of Gennari's line has therefore come to be known as " The external line of Baillarger." In 1855 Baillarger (*2*) published a second contribution in which he reaffirmed his six layers of cortex and once more drew attention to a stratum of white matter deeper than Gennari's line which is usually referred to as " The internal line of Baillarger," corresponding with the inner margin of the fifth cortical layer (Fig. 5). Baillarger therefore deserves the credit of having described the six cortical layers now recognized in the cerebral cortex. His contribution is the more remarkable since it was accomplished without the aid of a microscope. Baillarger was one of the foremost psychiatrists of France, an able follower of Pinel in supporting reform for the treatment of the insane, and

This further establishes Vicq d'Azyr's claim to independent discovery of the line, and had the proceedings of the academy been published promptly, Vicq d'Azyr could have claimed priority of publication. Vicq d'Azyr's plates published in 1784 do not show the striation mentioned in his text.

EXPLICATION DES PLANCHES.

Nota. Toutes les figures ont été dessinées sur les pièces fraîches, excepté les figures 1, 2, 3, de la planche première, copiées dans d'autres ouvrages. La figure 6 de la planche 1 et la figure 8 de la planche 2, sont des figures grossies. La figure 10 de la planche 2, vue, au microscope beaucoup plus grande, a été diminuée. Toutes les autres figures excepté la figure 11 de la planche 2, sont des lames minces de circonvolutions placées entre deux verres, et plus ou moins aplaties.

PLANCHE I.

Fig. 1. Ligne blanche dans l'épaisseur de la couche corticale vue par Vicq-d'Azyr, dans les lobes postérieurs, et par M. Cazauvieilh dans toute l'étendue des circonvolutions.

Fig. 2. Cette figure est copiée de l'ouvrage de Gennari. B. substance blanche A. A. couche mince d'un blanc jaunâtre placée entre la substance blanche et la substance grise. D. D, couche de substance grise séparant la ligne A A d'une seconde ligne blanche C qui se trouve par conséquent dans l'épaisseur même de la substance corticale. Cette disposition a été notée par Gennari comme exceptionnelle.

Fig. 3. Autre figure copiée de l'ouvrage de Gennari représentant la disposition qu'il a vue en général. B, substance blanche, A, A, A, troisième substance du cerveau formant une couche mince entre la substance blanche centrale et la substance corticale, laquelle, par conséquent, n'est pas divisée.

Fig. 4. Circonvolution du cerveau du mouton. On voit les six couches alternativement grises et blanches.

Fig. 5. La même vue par transparence. Les couches blanches étant opaques sont marquées en noir; les couches grises transparentes sont marquées en blanc. C'est donc le contraire de la figure 4; ainsi la première couche grise dans la figure 4 est blanche dans la figure 5 et ainsi des autres. On verra mieux cette disposition dans la figure 6.

Fig. 6. Figure grossie. La moitié gauche est vue par l'aspect simple, la moitié droite est supposée vue par transparence. Moitié gauche: B, Substance blanche, 1, 2, 3, 4, 5, 6. Les six couches alternativement grises et blanches.

Moitié droite: B. Substance blanche vue en noir parce qu'elle est opaque. 1, 2, 3, 4, 5, 6. Les six couches alternativement transparentes et opaques. Les couches transparentes marquées en blanc 1, 3, 5, se continuent avec les couches grises 1, 3 5, de la moitié gauche marquées en noir; les couches opaques 2, 4, 6, vues en noir, se continuent avec les couches blanches 2, 4, 6, de la moitié gauche.

Fig 7. Circonvolution du cerveau de l'homme. Aspect simple. Les six couches alternativement grises et blanches.

Fig. 8. Variété dans une circonvolution du cerveau de l'homme, vue par transparence. Au milieu de la substance corticale, on voit une couche opaque formée des couches 2, 3 et 4, très-rapprochées. Au milieu de cette couche quelques intervalles

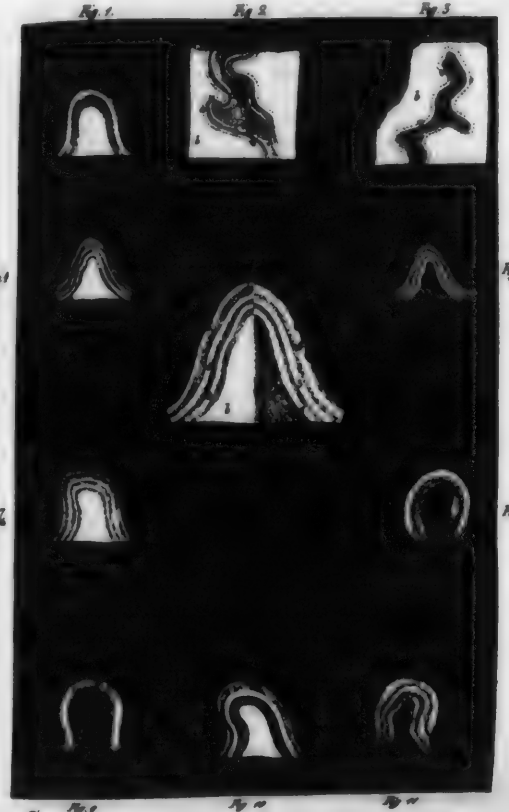

Fig. 4.

Baillarger's plate of 1840 with legends (left) showing the six cortical layers as made out in thin unstained slices of cortex viewed through glass. The upper two figures on the right are taken from Gennari's plate (Fig. 2 above). (*I*, Plate 1).

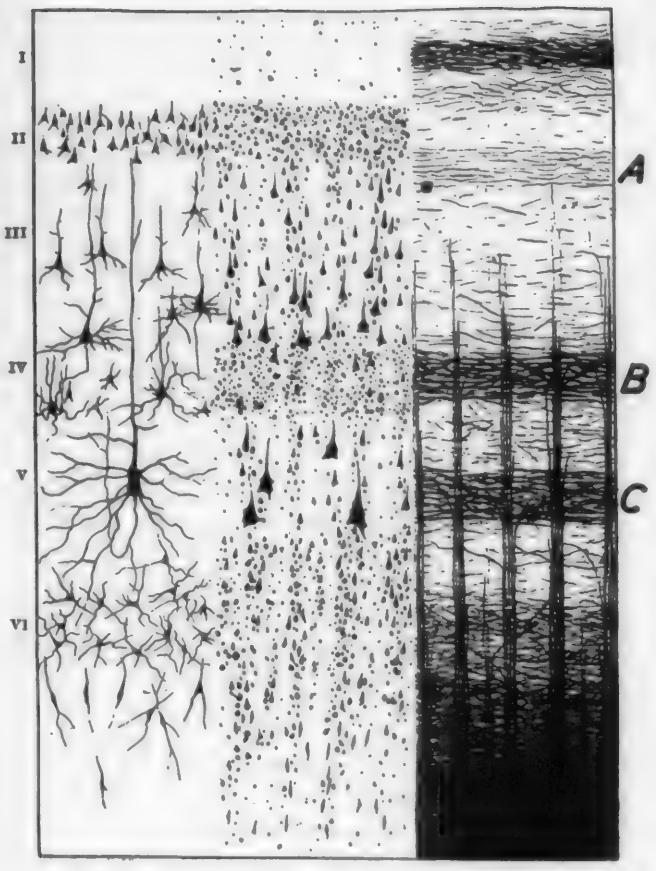

Fig. 5.

A modern diagram of the cortical laminae based on Golgi (left), Nissl (middle), Weigert (right) techniques of staining, showing the six histological layers of the cerebral cortex including: *B*—the lines of Gennari (External stria of Baillarger) in layer IV; *C*—the internal stria of Baillarger in layer V; and *A*—the fainter line of Kaes-Bechterew at the outer limit of layer III.

a pioneer of that still small band of those who believe that the major psychoses have a basis in anatomical disorganization of the brain. The details of his productive career are described by Semelaigne (*36*, tom. 1, pp. 332-342).

Early Microscopical Studies

Albrecht von Kölliker (1817-1905), in the second volume of his *Mikroscopische Anatomie* published in 1854 (*28*), had described the principal laminae of the cortex in unstained microscopical sections of the fixed brain. Four layers were recognized which he described from within out as an overlying white layer, a gray layer, an inner white layer, and finally an innermost gray-red layer. He had thus fused the outer two layers of Baillarger into one, and had similarly combined the inner two layers of Baillarger. Rudolf Berlin (1833-1897), in an Erlangen dissertation of 1858 (*4*) introduced the use of Gerlach carmine for study of cortical laminations; he was able to distinguish Baillarger's six layers, and in addition to describe the three principal cytological elements, namely: pyramidal cells, spindle cells and granular cells. This was the first important contribution to *cyto*architecture *per se* of the cortex.

Theodor Meynert (1833-1892), the Viennese psychiatrist, was the first to make detailed regional and comparative studies of cerebral cytoarchitecture which form the basis of all modern work in this field. His original papers appeared in 1867-68 in an obscure and short-lived journal (*30*). He recognized five, rather than six layers, as the pattern of generalized distribution in the neocortex; layers II and III of Baillarger were fused so that Meynert's fourth layer of motor cells is equivalent to layer V of Baillarger and most modern writers. Meynert subdivided the brain into two principal parts, one the archipallium of which the outer cell layer was made up principally of white fibers, and the neopallium which was more highly laminated and in which the outer layer was made up principally of gray matter. This was the first time cytological distinction had been given to the neopallium and the archipallium. He also recognized that the sensory areas of the cortex contained huge masses of granular cells in the fourth layer (his third layer, *31*),

and he described the principal association fibers of the cerebral cortex (" radiations of Meynert "; see Campbell, *14*, p. 25).

The discovery of the characteristic histology of the motor cortex is of some interest. Meynert seems to have missed the giant cells of the motor region, but in 1874 Betz, a Russian anatomist, saw them in man, and found that similar cells occurred anterior to the cruciate sulcus in the dog (*7*), the region, as he pointed out, in which Fritsch and Hitzig a few years earlier had obtained motor responses. No illustrations, however, are given in Betz' communication. In his next papers (*8*), a series of three published in 1881 and also unillustrated, Betz lays great emphasis upon the laminar structure of the cortex, recognizing the five layers of Meynert and still placing his motor cells in the fourth, or next to the deepest, layer. He states that he had examined 5000 preparations, individuals of all ages including a seven-months embryo, newly-born, a six weeks old child, etc. He described the distribution of the cells, and observed changes in cell structure which occur with age, and much else; indeed an extraordinary fund of detailed information is packed into these three papers, which together extend to about ten printed pages. It is stated by von Economo and Koskinas (*16*) that Betz contemplated an illustrated atlas of the human brain, but this was never completed. Vladimir Betz was born in 1834 at Kiev. He began his academic career as prosector in anatomy there, and held the chair of anatomy at Kiev from 1868 to 1889. He died September 30, 1894.

The Modern Period

The next important contribution was that of William Bevan-Lewis (1847-1929). In the first volume of *Brain*, published in 1878, Bevan-Lewis (*5*) *gives the earliest illustrated description of the giant cells of the precentral convolution*, which he called large " ganglion cells." He accepted the five-layer cortex of Meynert and therefore placed the motor cells in the fourth layer. Of Betz' observations Bevan-Lewis remarks (*5*, p. 80):

> " Another highly important feature of this [motor] region is the presence of large ganglionic cells which under the title of 'giant cells' were made the subject of special attention by Professor Betz over three years ago. Our examinations tend to convince us that these cells have a motor significance,

and that in their configuration, size and distribution, they present us with a thoroughly unique formation. These great elements are constituents of the fourth cortical layer; and as the question with regard to lamination centres around the mid-regions of the cortex, this fourth layer will necessarily share the larger amount of our attention."

Bevan-Lewis studied the dimensions of these cells in man and cat, and described the diminution in size of the cells situated more laterally in the hemisphere as contrasted with those occurring more medially. He and Clarke (*6*) also gave convincing proof of the homology of these cells in man with the large pyramidal cells found near the cruciate sulcus in dog and cat. With Betz he concluded that they were motor in function.

Bevan-Lewis was born at Cardigan in England, educated at Guy's Hospital and at the early age of 21 took his qualifying degrees to practise medicine in 1868. He joined the staff of the West Riding Lunatic Asylum, and he describes himself in his paper in *Brain* as "Pathologist and Assistant Medical Officer" to this institution. There he had contact with David Ferrier, Clifford Allbutt, Crichton Browne and many others of that group which made such notable contributions to cerebral physiology in the seventies and eighties of the last century. In 1884 he was made medical director of the Asylum. Later he was appointed professor of psychiatry at Leeds, a post he held until just before his death in 1929. He published two books, *The human brain histological and coarse methods of research*, 1882, and *A text-book of nervous diseases*, 1889. (Obituaries to be found in the *Lancet*, 1929, *2*, 954; the *British Medical Journal*, 1929, *2*, 833).

The next important contribution in the field of cytoarchitecture was that of a young Swede named Carl Hammarberg (1865-1893) who died just before his book (*23*) was to be issued. He adopted the six-layer scheme of Baillarger and extended the cytoarchitectural descriptions of certain regions which Meynert had only briefly touched upon. Hammarberg was the first to make a clear distinction between the cytoarchitecture of sensory and motor regions.

From the point of view of experimental physiology, the two most important cytoarchitectural studies are those of Alfred W. Campbell and the Armenian anatomist, Korbinian Brodmann, who worked

under the Vogts. Through contact with Professor C. S. Sherrington, Campbell had had opportunity to study in detail the anthropoid brains which Sherrington and Grünbaum had used in 1901 for analysis of the excitability of the cortex (See Grünbaum and Sherrington, *21*, *22*). These brains included those of orangs and chimpanzees, and detailed comparisons were made of these forms with human brains. Campbell is the only writer so far who has made a detailed analysis of the cytoarchitecture of the chimpanzee brain. A six-layer cortex was recognized, homologous layers were all described in considerable detail and an attempt was made to correlate cytoarchitecture with myeloarchitecture. His finely illustrated monograph (*14*) published in 1905 is one of the principal classics of neurological literature. Alfred Campbell left England for Australia shortly after his book was published and according to the British *Medical Directory* (1936) he is still in practice in Sydney.

It is beyond the scope of this note to deal with the history of myeloarchitecture, and it is sufficient to point out that the more important contributions were made by Meynert (*30*, 1867) and Flechsig (*18*, 1898) Exner (*17*, 1881) and a particularly notable series of papers by Theodor Kaes (*26*, 1891-1894) whose detailed monograph was eventually published in 1907 (*27*). He described the line of white matter forming the outer margin of layer III (Fig. 5). This band was independently described by Bechterew (*3*) and is known as the Kaes-Bechterew line. Cajal (1852-1934) studied all areas of the cortex systematically from the point of view of myeloarchitecture, his figures being perhaps more widely reproduced than other illustrations of myeloarchitecture (*33*). Finally Oskar and Cécile Vogt made detailed correlations between cyto- and myeloarchitecture in many primate forms (*41*, *42*, *43*).

Korbinian Brodmann began his studies (*9*) under the Vogts in Berlin in 1903. There followed a series of comparative studies (*10*, *11*, *12*) on the cytoarchitecture of the cortex which culminated in 1909 in the publication of Brodmann's well-known monograph *Vergleichende Lokalisationslehre der Grosshirnrinde in ihren Prinzipien dargestellt auf Grund des Zellenbaues* (*13*). Brodmann introduced a numerical scheme for designating the principal cytoarchitectural areas which has been followed in nearly all experimental studies of

the cortex. Brodmann extended his studies to man but did little with the anthropoid apes; in general his findings harmonize with those of Campbell. He recognized 52 discrete areas in the human brain; his studies have aroused more interest in cytoarchitecture *per se* than any previous investigator's.

After Bevan-Lewis and Campbell the work of the Vogts (*42*, *43*) on the cytoarchitecture of the monkey brain (1909, 1919) represents the most important attempt to correlate physiological observations with regional cellular organization of the cortex. The distribution of excitable foci was plotted on Brodmann's cytoarchitectural areas, but unfortunately the work was cut short before they attempted regional ablations in terms of Brodmann's areas.

For the human brain there are also ingenious studies by Elliot Smith (1909) and the now classical work of Economo and Koskinas (1925). Elliot Smith rediscovered Baillarger's procedure for examining thin slices of fresh cerebral cortex and he found by this simple method that he could distinguish some 28 different regions, nearly as many as Flechsig had by analysis of myeloarchitecture. To quote Elliot Smith's description (*37*, p. 239):

> "The fact remains that there are obvious macroscopic distinctive features of the various cortical areas: these I have attempted to map out. The fact that most of these areas have precise relations to various stable sulci, and that their distribution agrees so nearly with those mapped out by Flechsig, using another method, seem to point to the condition that we are dealing with truly localised cortical areas which expand at unequal rates in the course of their development, and are, therefore, presumably of different function.
>
> "And when I add that by the grosser method it is possible to do the mere mapping out of a hemisphere in one hour, which would take an immeasurably longer time to do by either of the other methods, we can appreciate how useful this method becomes when it is necessary to examine large series of specimens to determine the relationships of sulci and localised areas."

Economo and Koskinas issued a monumental treatise with an atlas of 112 plates illustrating the principal areas of the cortex in which they attempted to harmonize previous work, especially that of Brodmann with their own. They made an entirely independent description of the cortical areas, however, and introduced a modified system of lettering to indicate the primary field, recognizing more

than twice as many areas as had Brodmann and Elliot Smith. They believed that each one was a functional entity. In the most recent account of cytoarchitecture of the human cerebral cortex (*35*), that of Rose published in 1935, the schemes of designation of von Economo and Brodmann are combined. Marion Hines, the leading authority in this country in cytoarchitecture, remarked in referring to von Economo and Koskinas (*25*, p. 27):

"Using the six-layered cortex of previous authors and the cellular classification of Berlin, and benefitting by the great work of Brodmann, Economo and Koskinas brought to their study of cyto-architecture such a refinement, critical ability and thoroughness of observation that their atlas and descriptions (1925) will remain the classic for many years to come.

"It is a constant source of surprise to find an appreciable similarity between the descriptions of Brodmann and of Economo and Koskinas, in spite of the fact that the latter authors identified more than twice as many areas as Brodmann had."

Summary

Evidence of regional differentiation in the cerebral cortex was first recognized by a medical student of Parma, Francesco Gennari, who in 1776 observed a white band in the calcarine cortex of frozen human brains. The line was also independently seen in 1786 by Vicq d'Azyr and in 1788 by Thomas Soemmering, but considerations of priority warrant the designation "the line of Gennari." In 1840 Jules Baillarger, the French psychiatrist, on examining thin sections of fresh cerebral cortex held between two pieces of glass, recognized six cortical laminae, and identified the line of Gennari as forming layer III (actually the outer part of layer IV), not only in the occipital cortex, but in less conspicuous form in all parts of the cortex. The Gennari-Baillarger line has since been referred to as "The external line (or stria) of Baillarger," and an inner stria of the Vth layer as "The internal line of Baillarger" (See Fig. 5).

Berlin in 1858, using Gerlach's carmine as a stain, recognized the three principal cell types: pyramidal cells, spindle cells and granular cells; and in 1867-68, Theodor Meynert gave the first satisfactory comparative description of the principal cytoarchitectural fields in terms of the laminar differentiation; he also described the larger association systems within the cortex ("Radiations of Meynert").

The large cells of fifth layer of the precentral convolution of man were first described by the Russian neurologist, Vladimir Betz, in 1874, and first illustrated by William Bevan-Lewis in the initial volume of *Brain* (1878). Both Betz and Bevan-Lewis realized that the cells must be motor in function and that they were peculiar to the excitable region of the cortex described by Fritsch and Hitzig.

The later studies of Hammarberg (1895), A. W. Campbell (1905), Brodmann (1903-1909), Elliot Smith (1907), Kaes (1891-1907), the Vogts (1902-1919), and of von Economo and Koskinas (1925) are briefly touched upon.

APPENDIX

As Gennari's book appears to be one of the rarest documents in the history of modern neurology it has seemed desirable to give detailed bibliographical description of its contents. Baillarger had seen the book so there is undoubtedly a copy in France. Marimò in 1900 presented a copy to the Medical Faculty at Parma, stating that it was a great rarity; there is a copy in the Army Medical Library, Washington, D. C., from which the following description is taken:

Title: Francisci Gennari / Parmensis / Medicinae Doctoris Collegiati / De peculiari / structura cerebri / nonnullisque ejus morbis. / [rule] / Paucae aliae anatom. observat. / accedunt. / [device] / Parmae / [waved rule] / Ex Regio Typographeo / M. DCC. LXXXII. / Cum approbatione.

Collation: 4°: [*]^{1} *^{4} **^{4} *a*-*l*^{4}; 1 *l.*, xvi, 87 [1] pp., 4 pl.

Contents: [*]1*a* title; [*]1*b* blank; *1*a*-**4*b* (pp. i-xvi) dedication to Count Giuseppe Camuti; a1*a*-k4*a* (pp. 1-79) text; k4*b* blank; *l*1*a*-4*a* (pp. 81-87) explanation of plates; *l*4*b* blank.

Illustrations: There are 4 folding plates evidently on copper. Plate II is reproduced in the text above as Fig. 2. There is no indication of who executed the plates, although Gennari twice mentions his master Girardi in connection with the recently published plates of Santorini. The workmanship of the engravings is rather primitive, especially so when compared with the work of Mascagni then appearing at Pisa.

Note: The book is printed in an early Bodoni type, and bears the imprint "Ex Regio Typographeo" which Bodoni used during his early years. Bodoni was active at Parma from 1768 on, but his own name did not begin to appear on his imprint until 1791. Gennari's Latin has been proclaimed above reproach and his dedication to the famous archiatro Camuti is an elegant historical defence of anatomy. In referring to his master Girardi who had given him so many cadavers

he used the Cornelian eulogy of Themistocles: "No one was superior to him and few have been his equal."

Giuseppe Camuti (1730-1800) was a powerful figure of the time: scholar, physician to the court, professor of chemistry and rector of the university, he did much to foster the progress of Italian medicine.

Copies: Army Medical Library, Association Medico-Chirurgica of Parma, Fulton (Davis & Orioli, Oct. 1937).

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A NEW AND MORE CORRECT VERSION OF THE VIEWS OF SERVETUS ON THE CIRCULATION OF THE BLOOD

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During the last century a strong nationalistic movement has been in progress in Spain, tending to show that the fundamental discoveries on which modern knowledge of the circulation of the blood is based, were made by Spanish physicians who anticipated Harvey.

The movement was started in favor of Servetus by the Benedictine monk Fray Benito Jerónimo Feijóo (1676-1764), well-known for the persevering manner in which for more than half a century (in his *Teatro Crítico* from 1726 to 1740 and his *Cartas Eruditas* from 1741 to 1760) he attempted to dissipate the common errors and prejudices of his times and to propound reformation in medical teaching by suppression of its speculative nature and its liking for philosophical and theological disputes.

a) *Feijóo's Successive Opinions and their Grounds.*

At first Feijóo accepted Harvey's priority and referred to him as the "noble discoverer of the circulation" (*Teatro Crítico*, 1726, Prologue). Some years later (*Teatro Crítico*, Vol. IV, p. 315) he gave Cesalpinus the credit for the discovery, and charged Harvey with having ambitiously attempted to deprive him of the just fame. He declared, furthermore, that in the *Trevoux Mémoires* for the year 1737, it was asserted that Baron von Leibnitz apprehended for certain that "that famous antitrinitarian heretic Miguel Servetus had been the true discoverer of the circulation of the blood." Later on (*Cartas Eruditas*, Vol. III, p. 314) he once more changed his opinion in favour of a passage in Francisco de la Reina's "Libro de Albeytería" 1564) which had been shown him.

It has been recognized that Feijóo's trend of mind was the result of a chronic delight in discovering forgotten glories, of which some were warmly and undeservedly praised by him. Some have more recently been recognized as vain and unsubstantial (Marañón, pp.

113 and 116). Concerning his knowledge of Harvey's work on the circulation of the blood, we have demonstrated elsewhere (Izquierdo, page 218) that he never quoted it correctly as he did with Cesalpinus, probably because he knew Harvey only through second-hand references. Feijóo's quotation of Leibnitz's opinion deserves more particular attention because this quotation has been very generally taken for granted among medical historians (See Mariscal, p. 17; Marañón, p. 115; Goyanes, p. 211) to be a proof of Servetus' priority in the discovery of the circulation. As a matter of fact, what the *Trevoux Mémoires* asserted in the quoted passage (See

cencire Il ſe plaint de la rigueur dont on uſa contre Servet. *J'ai d'autant plus de compaſſion*, continuë-t-il, *du malheur de Servet, que ſon mérite devoit être extraordinaire, puiſqu'on a trouvé de nos jours, qu'il avoit une connoiſſance de la circulation du ſang, qui paſſe tout ce qu'on en trouve avant lui.* Il n'en étoit pas moins

Fig. 1.

Facsimile of the passage in *Mémoires de Trevoux* (1737, Vol. IV, p. 1966) unwarrantedly used by Feijóo to prove his assertion that Servetus was the true discoverer of the circulation.

Fig. 1) was only " that Servetus had exhibited knowledge which surpasses everything that one finds before him " (*Mémoires de Trevoux,* 1737, Vol IV, p. 1966). But the degree was neither accurately expressed, nor valued sufficiently to give Servetus the position of a " true discoverer."

b) *Where and how Servetus was led to express his views on the Circulation of the Blood.*

Miguel Servetus (1511-1553) undoubtedly deserves a prominent place among those brilliant men of his century who turned most vehemently against mere traditional knowledge. The nonconformity he felt, mainly on theological grounds, urged him to write and to print secretly his work *Christianismi Restitutio* (1553), for which

lem, quam nunc audies. Hinc dicitur anima esse in sanguine, et anima ipsa esse sanguis, siue sanguineus spiritus. Non dicitur anima principaliter esse in parietibus cordis, aut in corpore ipso cerebri, aut hepatis, sed in sanguine, vt docet ipse Deus genes. 9. Leuit. 17. et Deut. 12.

Ad quam rem est prius intelligenda substantialis generatio ipsius vitalis spiritus, qui ex aëre inspirato, et subtilissimo sanguine componitur, et nutritur. Vitalis spiritus in sinistro cordis ventriculo suam originem habet, iuuantibus maxime pulmonibus ad ipsius generationem. Est spiritus tenuis, caloris vi elaboratus, flauo colore, ignea potentia, vt sit quasi ex puriori sanguine lucidus vapor, substantiam in se continens aquæ, aëris, et ignis. Generatur ex facta in pulmonibus mixtione inspirati aëris cum elaborato subtili sanguine, quem dexter ventriculus cordis sinistro communicat. Fit autem communicatio hæc non per parietem cordis medium, vt vulgo creditur, sed magno artificio a dextro cordis ventriculo, longo per pulmones ductu, agitatur sanguis subtilis: a pulmonibus præparatur, flauus efficitur: et a vena arteriosa, in arteriam venosam transfunditur. Deinde in ipsa arteria venosa inspirato aëri miscetur, exspiratione a fuligine repurgatur. Atque ita tandem a sinistro cordis ventriculo totum mixtum per diastolem attrahitur, apta supellex, vt fiat spiritus vitalis.

Quod ita per pulmones fiat communicatio, et præparatio, docet coniunctio varia, et communicatio venæ arteriosæ cum arteria venosa in pulmonibus. Conderet hoc magnitudo insignis venæ arteriosæ, quæ nec talis, nec tanta facta esset, nec tantam a corde ipso otio purissimi sanguinis in pulmones emitteret, ob solum eorum nutrimentum, nec cor pulmonibus hac ratione seruiret: cum præsertim antea in embryone soleret pulmones ipsi aliunde nutriri, ob membranulas illas, seu valuu

valuulas cordis, vsque ad horam natiuitatis nondum apertas, vt docet Galenus. Ergo ad alium vsum effunditur sanguis a corde in pulmones hora ipsa natiuitatis, et tam copiosus. Item a pulmonibus ad cor non simplex aër, sed mixtus sanguine mittitur, per arteriam venosam: ergo in pulmonibus fit mixtio. Flauus ille color a pulmonibus datur sanguini spirituoso, non a corde. In sinistro cordis ventriculo non est locus capax tantæ, et tam copiosæ mixtionis, nec ad flauum elaboratio illa sufficiens. Demum, paries ille medius, cum sit vasorum et facultatum expers, non est aptus ad communicationem et elaborationem illam, licet aliquid refudare possit. Eodem artificio, quo in hepate fit transfusio a vena porta ad venam cauam propter sanguinem, fit etiam in pulmone transfusio a vena arteriosa ad arteriam venosam propter spiritum. Si quis hæc conferat cum iis, quæ scribit Galenus lib. 6. et 7. de vsu partium, veritatem penitus intelliget, ab ipso Galeno non animaduersam.

Ille itaque spiritus vitalis, a sinistro cordis ventriculo, in arterias totius corporis deinde transfunditur, ita vt qui tenuior est, superiora petat, vbi magis adhuc elaboratur, præcipue in plexu retiformi, sub basi cerebri sito, in quo ex vitali fieri incipit animalis, ad propriam rationalis animæ sedem accedens. Iterum ille fortius mentis ignea vi tenuatur, elaboratur, et perficitur, in tenuissimis vasis, seu capillaribus arteriis, quæ in plexibus choroidibus sitæ sunt, et ipsissimam mentem continent. Hi plexus intima omnia cerebri penetrant, et ipsos cerebri ventriculos interne succingunt, vasis illa secum complicata, et contexta feruantes, vsque ad neruorum origines, vt in eos sentiendi et mouendi facultas inducatur. Vasis illa miraculo magno tenuissime contexta, tametsi arteriæ dicantur, sunt tamen fines arteriarum, tenden-

Parte illa maiore inspiratæ aër per trachean arteriam ad pulmones ducitur, vt ab ipsis elaboratus ad arteriam venosam transeat, in qua flauo et subtili sanguini misceatur, ac magis elaboratur. Deinde totum mixtum a sinistro cordis ventriculo diastole attrahitur, in quo fortissima et viuifica ignis ibi contenti virtute, ad suam formam perficitur, et fit spiritus vitalis, multis in ea elaboratione expiratis fuliginosis recrementis. Hoc totum veluti materia est ipsius animæ. Vltra totum hoc mixtum, duo in anima superfunt: quid viuens spiratione crescat, aut in sua materia productum: et spiritus ipse, seu diuinitas ipsa spirando insita, omnia vnum, et anima vna. Id medium, quod principaliter anima dicitur, halitus est et spiritus, vtrinque cum spiritu essentialiter iunctus. Substantia est ætherea, illi archetypæ superelementari, et huic quoque inferiori similis: naturalis anima vna, vitalis et animalis. Ecce totam animæ rationem, et quare anima omnis carnis in sanguine sit, et anima ipsa sanguis sit, vt ait Deus. Nam afflante Deo, inspirata per os et nares, in cor et cerebrum ipsius Adæ, et natorum eius, illa cæleftis spiritus aura, siue idealis scintilla, et spiritali illi sanguineæ materiæ intus essentialiter iuncta, facta est in eius visceribus animæ. Gen. 2. Esa. 57. Ezech. 37. et Zacha. 12. Hoc modo tam dissimiles substantias posse coire, post Chaldæos docuerunt Academici, qui Aetheream quandam a Deo auram elementari aëri coniungi dixerunt, vt eo modo in denfum hoc corpus diuina mens immitti possit. Docent id clarius sacræ literæ, halitum Dei, et elementarem flatum passim vocantes. Plato in Timæo aperte docet, substantiam animæ esse elementaris et diuinæ substantiæ commixtionem quandam. Esse tertiam quandam substantiam mediam, vtriusque participem. Nam continet anima symbolum Deitatis et mundi elementorum. Alioquin non posset anima vna vires illas intelligibilis mentis habere, et viuificas has rectrices corporum

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Fig. 2.

The three pages from *Christianismi Restitutio* (1553) with Servetus' views on blood circulation. Photostats from the Murr reprint, 1790.

the great religious intolerance of his time demanded his life as the payment.

In this book he discussed the problem of the soul and engaged in bold disquisitions about it. This was, for him, the point of entry into the more important physiological problems of his day (Pp. 170, 171 and 178; see Fig. 2).

Quaint old views found by him in the Hebraic books (Genesis, Leviticus, Deuteronomy), which were coincident with those of the ancient Greek physicians, were the starting point from which he raised his main argument: that the soul was in the blood (*Chr. Rest.*, p. 170). He maintained (P. 178) that the soul had been infused by God into the blood of the first man precisely through the same route as that by which the ancients thought the *pneuma* passed from the surrounding air down to the right ventricle of the heart. The fact that the spirit or *anima* could persist afterwards in the blood, he attributed to its constant regeneration, occurring as a result of the contact between blood and *pneuma*. This was the reason he sought to explain where and how this contact took place.

In his exposition Servetus conformed very closely to Galen's doctrine which he knew thoroughly. Nevertheless, he introduced a number of alterations which, although originally very clearly enunciated by him, in later years have been misunderstood, overestimated in their reach, and forced or changed in their meaning. All this was the result of different sorts of passions and superficiality, or lack of knowledge of the opinions of the ancients.

Consequently, in order to examine Servetus' thoughts more closely, the first thing that should be done is to study his actual opinions and to separate them from those merely attributed to him by authors who paid more attention to their own fantasy than to Servetus' own words. At the same time, we must recognize which of his opinions varied from those of the ancients. Some of them have been considered different, only because of defective knowledge of the medical writings of the past.

It is our hope, in performing this task, that the spirit of calm justice and impartiality with which we have planned to proceed, will be recognized.

In the following sections we shall limit ourselves to proving,

firstly, what were the new and original opinions of Servetus concerning the circulation of the blood, and secondly, on what evidence they were based and by what methods he was able to reach his conclusions.

c) *Servetus showed for the first time a communication between the Sanguineous and Aerial systems of Galen.*

Fundamentally, Servetus' innovation consisted in maintaining that the most important channels of communication between the two independent systems of Galen were not the porosities fantastically located by him in the interventricular septum, but some other openings in the lungs which formed an *artificium magnum*. He compared them to those openings in the liver by which blood is transferred from the vena porta to the vena cava (*Chr. Rest.*, p. 171). Through these openings in the lung, he considered that blood filtered from the vena arteriosa to the arteria venosa. He recognized also that a small amount of blood passed through the septum, but held that rather unimportant, because the substance of the septum appeared to him as " deprived of uses and faculties " for it (p. 171).

It has been asserted (Goyanes, p. 210) that Servetus was the first to declare that the opening " in the septum between both ventricles became obliterated at birth," but he seems rather to have referred to the closing of the interauricular septum already known to Galen, as well as to the occlusion of the *ductus arteriosus*. (*De Usu Partium*, XV, p. 213; see Fig. 3.)

So far, there have been no difficulties among authors in admitting that Servetus pointed to a new communication between the two Galenic vascular systems which was different from Galen's " synanastomosis." But when the authors are consulted concerning the quality and quantity of the substance which Servetus considered as passing through the new channel of communication, it is found that they are far from accord.

d) *Servetus Asserted that Blood Passed Through a Channel which he Described for the First Time.*

It has been said that Servetus accepted the passage of only a *spirit* or *anima* through the new channel. As the basis for this

Etenim,q̃ primum animans in lucẽ eſt æditum,aut ante vnum vel duos dies, in quibus ſdam verò ante quatuor aut quinq; , vel non nunq̃ plures,membranam,quæ eſt ad foramen,coaleſcentem reperias,non dum tñ coaluiſſe : cum aũt animal perfectum fuerit,ætateq; iam floruerit,ſi locum hunc ad vnguem denſatum inſpexeris, negabis fuiſſe aliqñ tempus,in quo fuerit pertuſus:multo aũt magis,in ijs,quæ adhuc vtero gerun tur,aut in nuper genitis,membranam conſpicatus ad ſolã quidem radicẽ firmatam,reliquũ verò to tum corpus in valorum cauitate pendulum, exiſtimabis fieri non poſſe,vt ipſa vnquã perfecte coa leſcat.Corpora ſanè neruoſa ac tenuia,etiam ſi quis repente,atq; vbi primum fuerint diuiſa,glutina re aggrediatur,voti compos non fit,{neq; magnopere cohærent,} nedum ſi iam pridẽ fuerint per fecta:atqui membrana illa tpe procedente omnino coaleſcit,non,q̃ neruoſa ſit ac tenuis,neq;,q̃ mo tu perpetuo agitetur,impedita.Pari modo id vas,quod magnam arteriam venæ,quæ fertur ad pul monem,connectit,cum aliæ omnes animalis particulæ augeantur,non modo non augetur , verum etiam tenuius ſemper effici conſpicitur adeo,vt tempore procedẽte penitus tabeſcat atq; exiccetur.

De Usu Partium, XV, page 213.

Proinde neq; proprius eius ſan guis ſecoris ſanguini ſimilis eſt ſed quo calidius viſcus viſcere,eo flauior ſanguis conſpicitur,quem q̃lorem ſiccioris ſanguinis,ſicut humidioris ruborem proprium eſſe conitat. De quibus quidem coloribus etiam Plato in Timæo ſcriptum reliquit. Nos verò eius orationem explanantes , veram eſſe demonſtrauimus alio in libro , quem de coloribus peculiariter compoſuimus . ſed nunc rem propoſitam abſoluamus.Sanguinis temperatura humidi,colore verò rubri prima in iecore genera tio efficitur vehicula, à quibus deriuatur,diſtribuiturq; in totum corpus,venæ habentur : at flaui , & tenuis , ſubulisq; ſubtilibus ſanguinis alterius , & ſpirituoſi in læuo cordis ventriculo ortum eſ ſe non dubitamus , quem quidem genus ſanguinis in totum corpus poſtea deuehunt arteriæ , ac di ſpenſant, id quod diligentius in cõmentarijs de facultatibus naturalibus oſtendimus.

De Hippocratis et Platonis Decretis, page 266.

Quãdoq; verò quod flauũ eſt,eo ſuper abũdat:vt id facile & ex capillis,& ex corporis colore,& præterea ex vomitu, & alui deiectionibus conſpicere poſſis.Non nunquam etiam id quod aut rubrũ eſt aut albũ,quæ & ipſa tum in colore to tius corporis,tum in vomitu & alui excremẽtis notari poſſunt. Quòd ſi ipſas etiã venas feces homi nibus qui proſpera adhuc fruuntur valetudine:ab his quidẽ flauus profluet ſanguis , ab alijs ruber, ab aliquibus nigrior,ab alijs albior.

De Elementis, page 9.

Fig. 3.

Facsimile reproductions of three of the passages from Galen, quoted in the text. Reproduced from the Venetian edition of Galen's Works, by Iuntas (1576).

assertion the passage has been quoted where he declared that " equal transfusion of the spirit is made from the *vena arteriosa* to the *arteria venosa*" (*Chr. Rest.*, p. 171). But to place a narrowly literal interpretation of these words is like pretending that today, by asserting that oxygen is transferred from arteries to veins, one means only that oxygen is carried, ignoring the fact that it is conveyed by the blood. In a similar way, it was but natural that Servetus who was strongly biased about the problem of the *spirit* and the *soul*, referred mainly to them without insisting any more on what he had already asserted: that both were within the blood.

Those who translated the color *flavus* which Servetus attributed to the blood after it passed through the lungs as *yellow* (Sénac, p. 78; Flourens, p. 29; Daremberg, Vol. III, p. 596; Dastre, p. 652), were forced to deny that Servetus could have been referring to blood. And it is quite interesting to discover that many among them were not in the least surprised that Servetus could be referring to a " yellow " or " golden " blood. Only Sénac, more conscious of the difficulty, made the supposition that Servetus imagined the passing " of a spirit or other golden substance."

As a matter of fact, Servetus did not intend to say that the blood could turn yellow in the lungs, nor was he the first to use the adjective *flavus* to qualify its color. The Latin translators of the writings by the old Greek physicians (See Felicianus version of Galen's *De Hippocratis et Platonis Decretis*, p. 263) used the word *ruber* to describe the color of the venous blood, and the word *flavus* for the spirituous thin blood, which they supposed to originate from the contact of the blood from the right ventricle, mixing with the air in the left ventricle. This they called also *flavior sanguinem* and sometimes *ruberrimus*. *Flavus* was used also to qualify the color of red hair (See Trincavelius version of Galen's, *De Elementis*, pp. 8-9).

By giving to *flavus* its real meaning of *light red*, there are no more difficulties in recognizing that Servetus referred to the passing of blood through the new passages in the lung.

To translate *flavus* as merely *red* (Flourens, pp. 23-29; Chinchilla, p. 35; Richet, p. 689; Comenge, p. 52; Goyanes, p. 210) is also inaccurate and it veils an important part of Servetus' view.

according to which the blood turned to a *light red* color after passing through. We shall return to this point later.

e) *Servetus Asserted that only a Small Fraction of the Blood Filters through the Lungs.*

For more than a century, authors have kept repeating (Feijóo, p. 37; Flourens, pp. 25 and 29; Hernández Morejón, Vol. II, p. 51; Comenge, p. 59; Béclard, pp. 216-217; Mariscal, pp. 67, 74, 75, and 81; Goyanes, pp. 209, 210 and 222) that Servetus asserted that the whole mass of blood passes through the lungs exactly as we know it does. Father Feijóo declared him " the true discoverer " of the pulmonary circulation. Fluorens considered him the first to give " a complete notion " of the blood circuit through the lungs, in which the blood flowing from the right ventricle " *returns back* into the left heart by the pulmonary vein and therefore forms a circuit," and considered his description to be of an accuracy " not to be surpassed later." To Hernández Morejón it not only seemed that Servetus had made the discovery, but that his explanation was so complete and clear " that nothing essential had been added to it afterwards." Similar opinions are expressed by contemporary writers, who still consider Servetus " the first to define, anatomically and physiologically, the pulmonary circuit " (Carbonell) and who even suppose that he made " an exposition of the haemodynamics in the small circuit " (Goyanes) at a time when neither he nor anybody else could have done so.

Now, omitting from the argument the interpretation of Servetus' words (*longo per pulmones ductu*; p. 170) " carried a long stretch through the lungs," wrongly translated as " circular motion " (As Carbonell, p. 461), the basis for all other assertions is only the fancy and carelessness of the commentators. Since it is true that Servetus still adhered to the Galenic view that the veins and arteries form two systems, respectively, which are independent of each other, it becomes evident that the idea attributed to him that the *blood returns* from *the right ventricle to the left,* is quite unwarranted. Servetus' own words (*in pulmone transfusio à vena arteriosa ad arteriam venosam propter spiritum*) express with certainty and beyond any doubt that the only fraction of blood he thought to pass

through the lungs, was the same small amount which, according to Galen, filtered from the sanguineous (venous) to the aerial (arterial) system. Although this is so clear, it is surprising to discover that up to the present it has not been recognized except by the scholarly and accurate historian of medicine, Professor Max Neuburger. I am indebted to Professor H. E. Sigerist for having called my attention to a short but important paper of his (Neuburger, p. 7), which apparently has not received all the attention that it deserves. Neuburger, too, believed that Servetus only expressed his views with reference to a small portion of blood, but never suspected that the whole mass of blood might filter through the lungs, and that this explains why Servetus did not discover the greater circulation (See Section g). Had he really grasped that the whole mass of blood passed through the lungs, it would be incomprehensible that he should not have immediately jumped to the idea of the systemic circuit. For then he would have been forced to admit—as Harvey did three-quarters of a century later—that the blood which was sent to the aorta and arteries from the lungs and the left ventricle, could originate only from the pulmonary artery, the right ventricle and the whole system of the veins. But Servetus could not make this harmonize with his Galenic faith that the blood was prepared in the liver and distributed to the whole body through the veins.

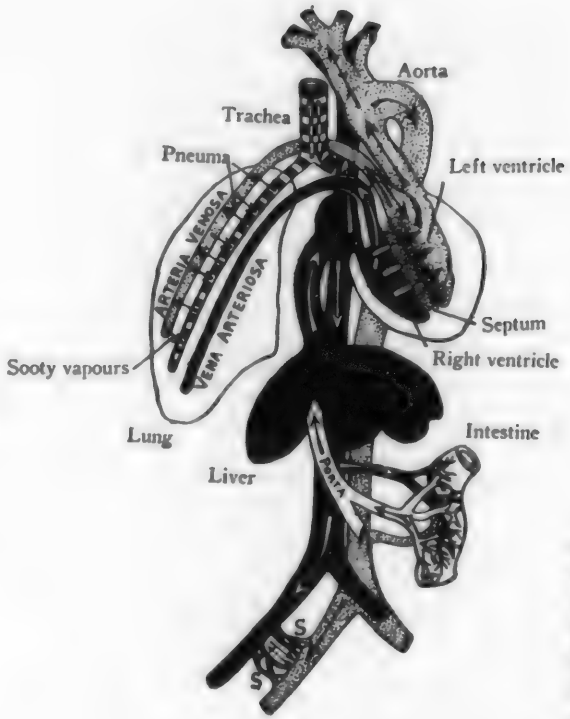
Consequently Servetus' conception cannot be taken as equivalent to the modern notion of the pulmonary circulation nor as the enunciation of the discovery of the point where dark blood turns bright, as has been repeatedly stated (Sénac, p. 78; Flourens, p. 27; Chinchilla, p. 35; Guardia, p. 391; Comenge, p. 52; Mariscal, pp. 77 and 81; Goyanes, p. 209). To claim that Servetus observed the phenomenon or learned it by mere intuition is indefensible. The grounds for Servetus' maintaining that the blood turns red in the lungs will be considered under Section h. Direct knowledge of the changes was gained considerably later by Richard Lower (1669), who demonstrated by experiment that the dark venous blood, when passed through the lungs, becomes bright in color and absorbs from the air a definite chemical substance necessary for life; and that this is, in fact, the chief function of the pulmonary circulation.

It is easy to confirm that Servetus remained completely a Galenist

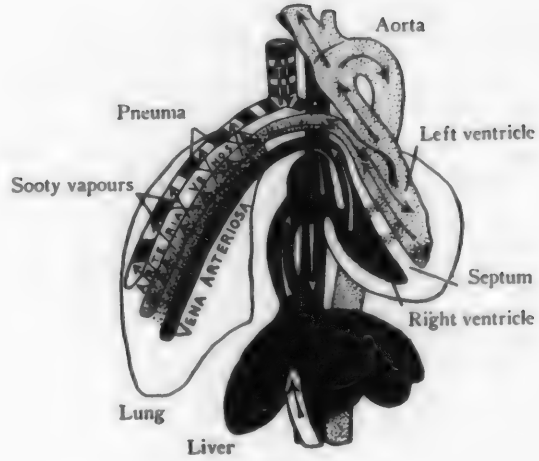
in his description. In the first place he was not the first, as it has been supposed (Goyanes, p. 210), to assert that the vena arteriosa (pulmonary artery) carries blood to the lung for its nutrition, because such a view had already been expressed by Galen.

He was more original in suspecting that the capacity of the vena arteriosa was rather excessive for such a purpose of nourishment (*magnitudo insignis venae arteriosae, quae nec talis, nec tanta facta esset, nec tanta à corde ipso vim purissimi sanguinis in pulmones emitteret, ob solum eorum nutrimentum*). Also he pointed to multiple communications between the vena arteriosa and the arteria venosa (*Quod ita per pulmones fiat communicatio, et praeparatio, docet conjunctio varia, et communicatio, venae arteriosae cum arteria venosa in pulmones*; Servetus, p. 170). Concerning the blood which he suspected to pass through the new channel into the arteria venosa, he recognized it as the same small amount which Galen assumed to flow from the venous to the arterial side for the generation of the vital spirit. Only he changed the seat of its passage: from openings in the medial septum to those in the lungs.

It cannot be pretended that Servetus "pulled down once for all, the error of the passage of air through the cavity of the modern pulmonary vein" (Richet, p. 691), or that his description "made vanish the imaginary to and fro movement of blood and air through the pulmonary veins" (Comenge, p. 52). He stated quite clearly the Galenic theory of the air (mixed with blood) sucked by the heart through the *arteria venosa* (*à sinistro cordis ventriculo diastole attrahitur*) (P. 178). And his description did not lack the assertion that the arteria venosa serves as the channel for the expulsion of the sooty or fuliginous vapours originating in the left ventricle (*Deinde in ipsa arteria venosa inspirato aeri miscetur, expiratione à fuligine repurgatur*; Servetus, p. 170). It has been assumed that the same idea "cannot be detected in any other writer before the Aragonian learned man" (Mariscal, p. 32). Nevertheless, it is enough to go through passages from the text of Galen as the one in the adjoined facsimile, to be convinced of the contrary. (See Fig. 3.)



GALENIC DOCTRINE



SERVETIAN DOCTRINE

Two schemes to illustrate and compare the Galenic doctrine on the blood circulation and the modifications introduced by Servetus.

Vessels in black represent the Galenic *venous* or *sanguineous system*, supposed to communicate with the trachea.

Dotted vessels correspond to Galenic *aerial* or *spirituous system*, supposed to communicate with the trachea.

According to Galen, the systems were independent and only centrifugal from their origin. Nevertheless, he admitted them to communicate (1) through porosities (represented by arrows) in the septum between ventricles, and (2) through " synanastomosis " (8) allowing communication either way in the periphery between arteries and veins.

Servetus still maintained the view that both systems were independent and he accepted the passage of a small fraction of blood through the interventricular septum.

But he admitted that filtration was far more important from vena arteriosa (modern pulmonary artery) to arteria venosa (modern pulmonary vein). It is to be noted that for him the arteria venosa was still part of the aerial system with the functions: (1) of conveying to the left ventricle the mixture of blood sucked by the heart during diastole and of blood filtering through the lungs, and (2) of collecting from the left ventricle and expelling through the trachea, the sooty or fuliginous vapours formed within its cavity.

Fig. 4.

f) *Of What Servetus' Modification of Galenic Doctrine Consisted.*

Once the fundamental points in Servetus' exposition are reduced to their real value, it will be much easier to appreciate his innovations in the Galenic doctrine with the aid of the adjoining schema (Fig. 4):

1. Servetus concluded that the small fraction of blood which Galen deemed necessary to pass from the sanguineous to the aerial system for the formation of the vital spirit, filtered through the lungs and not through the interventricular septum, as proposed by Galen.

2. As a consequence of having pointed to a new seat (lung) for the blood to come in contact with the air, Servetus had to refer to this new seat the color change which, according to Galen, was brought about by the contact of the blood with air.

3. Without denying the Galenic uses of the *arteria venosa* as a channel through which the aerial (left) ventricle aspirates air from the trachea and expels fuliginous vapours formed in its cavity, Servetus assigned to this vessel the supplementary function of conveying to the ventricle the small fraction of blood filtering into its cavity from the lung.

In short, *Servetus' modification of the Galenic doctrine consisted in having it ingeniously adapted to the new seat he had proposed for the communication between the sanguineous and the aerial systems.*

g) *General Circulation was Unknown to Servetus.*

Not long ago the view was still maintained (Mariscal, p. 70) that Servetus had described the systemic circulation, although he did not develop it " because he did not need it at all " (*Ibid.*, p. 77). Nevertheless, upon careful inspection of what Servetus stated (p. 171), it is clear that what he really described was the distribution through the arteries of the vital spirit supposed by Galen to originate in the left ventricle (*spiritus vitalis à sinistro cordis ventriculo in arterias totius corporis deinde transfunditur*).

The origin of the false supposition that Servetus knew of the systemic circulation is to be found in the inaccuracy or incompleteness of some of the versions given for the preceding paragraph.

Goyanes faithfully transcribed it on page 208 of his book, but in his Spanish version (P. 207) he omitted the reference to the material which Servetus asserted was distributed through the arteries, which afterwards in his exposition, he merely supposed to be blood.

Many other authors have translated *spiritus vitalis* as if it connoted arterial blood (Richet, p. 689; Comenge, pp. 52-54; Codorniú y La Rubia, T. II, p. 179; Mariscal, pp. 77-81; Goyanes, p. 214). But this is without sufficient grounds, because for Servetus as well as for every Galenist of his day, the vital spirit was not the blood, although for them blood was one of the elements composing the mixture they considered as filling the aerial system. Furthermore, Servetus expressed his views on the vital spirit so clearly on page 170 of his book (*spiritus tenuis, . . . quasi ex puriori sanguine lucidus vapor*) that there can be no doubt that he referred specifically to it, and not to the blood, from which he considered it to originate. It is still more unwarranted to hold that the substance he was discussing might have been arterial blood.

The supposition that Servetus might have known the systemic capillaries (Mariscal, p. 85; Goyanes, p. 210) is even less defensible.

h) *The Foundation of Servetus' Views and the Reasoning by which He Reached Them.*

It has been set forth in a number of papers published within the last few years (P. Diepgen, 1932; M. Meyerhof, 1933; S. I. Haddad and A. A. Kairallah, 1936) that El Korraschi, or Ibn Nafis expressed views in Damascus in the thirteenth century, which coincided strikingly with those defended by Servetus a century later. But for the present there is no basis for considering that either Servetus or any other European physician of that time had any knowledge of these writings.

For several decades it was repeatedly asserted, after Father Andrés (P. 406), that Servetus had derived all his information from the Spanish schools where he had been educated. As a matter of fact, Servetus did not receive his education in such schools (Mariscal, p. 406) nor were the latter so well-informed about the circulation as has been assumed (See Izquierdo, p. 227).

A number of writers have admitted the great value of Servetus'

views merely because of the ideas they thought they discovered either in Servetus' own words or in those attributed to him by his translators (Flourens, pp. 29 and 151). After reading his announcement that he was going to reveal "marvelous facts hidden from the greatest philosophers" they took these words as proof of his "full consciousness on his grandiose discovery" (Goyanes, p. 205), or as though they were doubtless the result of "having seen and seized the truth in the very entrails of Nature" (Mariscal, p. 25). Nevertheless, the emphasis with which words are uttered or written cannot be taken as testimony of the solidity of the assertions made by them, and this is especially true with regard to Servetus, who was always "disposed to exalt his merits and studies" (Goyanes, p. 209).

In order to reach a more firm conclusion, the chief interest is to ascertain the scientific value of Servetus' opinions from the standpoint of the strength and soundness of the foundations upon which he constructed them. Did he use for this purpose anatomical observations from dead or living bodies, or did he merely offer a product of his free imagination?

From the foregoing analysis it becomes evident that Servetus began with inferences drawn from anatomical data (impermeability of the septum; large capacity of the vena arteriosa; sigmoids open only after birth, when the lungs begin to be active). These few anatomical data were enough for him to ground his suspicion that beween the vena arteriosa and the arteria venosa there was a mechanism in the lung similar to that between the vena porta and the vena cava in the liver.

But to what degree were his anatomical observations based upon direct information? The greatest contradiction is found among the historians of medicine. Some of them ascribe to him great experience in dissections of cadavers (Codorniú, p. 179; Comenge, p. 51; Mariscal, pp. 24, 69 and 74; Carbonell, p. 462) or even suppose that he carried out his studies upon the battlefield (Mariscal, p. 24). But M. Roth (P. 247), well-known student of sixteenth century anatomy, concludes that Servetus' anatomy was more speculative and imaginary than real. The statement written by the illustrious anatomist Valverde (Back of p. II) thirteen years after Servetus'

death, "that it was a hideous thing among Spaniards to cut dead bodies into pieces" (See Fig. 5), makes his devotion to dissection doubtful. Whatever it may have been, we are not in a position to decide on the nature of his anatomical knowledge. Until new evidence is forthcoming, we must confine ourselves to the rôle played by anatomical inferences on function.

As for vivisection, the only inference which can be safely drawn, is that Servetus never practiced it. Of course, authors can be found

ONSIDERANDO Illuſtriſſ. Señor la gran falta, que la nacion nueſtra tiene de hombres, que entiendã la Anatomia: aſſi por ſer cola fea entre Eſpañoles, deſpedaçar los cuerpos muertos, como por auer pocos, que venidos â Italia, donde la podrian deprender, no huelguen antes de occuparſe en otros exercicios q̃ en eſte, por no eſtar acoſtumbrados, a ſemejantes coſas: y viſto el daño, que deſto ſe ſigue a toda la nacion Eſpañola, parte por los Cirujanos (a quien mas falta haze no entenderla) ſaber poco latin, parte por auer

Fig. 5.

Facsimile of Valverde's declaration on the repulsion felt by the Spanish physicians of Servetus' time for dissection of cadavers (Valverde, *Historia de la Composición del Cuerpo Humano*, Dedicatoria, p. II, back).

who have declared his *Christianismi Restitutio* "an account of something learnt and searched out by experimental methods" (Goyanes, p. 214), or something based "on objective and experimental facts" (*Ibid.*, p. 210) and conquered as the result of "convincing and definitive demonstrations" (*Ibid.*, p. 24). But of what value can all these assertions be, when it is impossible to show records of a single experiment among those which have been gratuitously assigned to him. It is quite curious to discover that in order to conceal this gap, additional suppositions have been added, to the effect that "his early death prevented our having notice of them" (Mariscal, p. 76), or that he did not put them down because

"he was not obliged to, since he merely explained circulation accidentally in a book devoted to theology" (Goyanes, p. 217). But the fact remains that in going over and over Servetus' text, no account is found of any experiments or observations made *in vivo* by him.

If due attention be given to the kind of interest which moved Servetus to write his *Christianismi Restitutio,* to his scholasticism and to the liking he showed for astrology during his early stay in Paris, one feels rather inclined to consider his work more as a product of imagination and fantasy, than as an achievement by scientific methods.

Consequently, we must deem incongruous, all those opinions in which Servetus has been proclaimed "sagacious physiologist," "predecessor of modern physiology" (Comenge, pp. 21 and 23) or "great physiologist" (Mariscal, pp. 74 and 134), "disorderly scattering about his discoveries through pages in which were included virtually all the discoveries afterwards made" (*Ibid.,* p. 78) because "there was almost no point that he did not observe or discover" (*Ibid.,* p. 81).

From all that has been said, it must furthermore be concluded that Servetus did not glimpse the new experimental method which Harvey introduced as the basis of modern physiology three-quarters of a century later (1628).

This is why, in proposing real innovations in the current physiological views of his day, he could not break away from the Galenic doctrine.

Summary and Conclusions

As the result of strong religious or nationalistic feelings; of not paying due attention to the writings by the ancients as a basis for comparison; or of indulging in accepting writings of the past as expressing modern ideas, Servetus' opinions on the movement of the blood have been repeatedly misunderstood, distorted or overestimated.

The author has therefore sought to discover which were really the views expressed by Servetus (See Fig. 4) and which were not, though often assigned to him, and what were the grounds and methods he used to reach his opinions.

A. *Opinions really expressed by Servetus*

Servetus adhered to the Galenic doctrine holding the venous (sanguineous) and aerial (spirituous) system as independent from each other and only communicating at certain points. Nevertheless, he introduced some original modifications.

a. Instead of accepting with Galen that the main passage was through the interventricular septum, he maintained that a more important channel of communication existed in the lung, between the vena arteriosa (modern pulmonary artery) and the arteria venosa (modern pulmonary vein).

b. The same fraction of blood which Galen considered to go through the interventricular septum in the heart, Servetus maintained to filter through the new channel for the same Galenic purpose of coming into contact with air and thus originating the *vital spirit*.

c. Consequently, the color change which occurred when the fraction of blood filtering from the venous side came into contact with air, was transferred by him from the original location given by Galen, to the new place where he said the blood filtered. That this was his opinion is confirmed by his assertions fully supporting the Galenic views upon the arteria venosa (wide open channel between trachea and left heart, through which air was sucked during diastole and sooty or fuliginous vapours were expelled in systole).

B. *Opinions Improperly Assigned to Servetus*

a. That he asserted passing of *the whole* mass of blood through the lungs. He only referred to a fraction, as explained above.

b. That he described the *pulmonary circuit*. He could never think that the blood in the mixture conveyed by the arteria venosa could be *returning to the heart to complete a circle*, because for him the right and left hearts were independent.

c. That he discovered pulmonary haematosis. He merely transferred that Galenic color change in the blood to the new place of its contact with air.

d. That he was the first to describe the closing of the opening in the interventricular septum. (See in Fig. 3 Galen's description of this).

e. That Servetus knew the systemic circulation. He only described the Galenic distribution of vital spirit from the left ventricle to the smaller arteries.

C. *The Evidence upon which Servetus Grounded His Views*

a. Inferences from anatomical information (the septum's thickness and impermeability; large capacity and branching in vena arteriosa; condition of sigmoids after birth). Nevertheless, information is lacking to decide if his anatomy were learned by dissecting cadavers, or only speculative.

b. Analogy with the passage through the liver between the vena porta and the vena cava.

c. No indications can be found which might uphold the supposition that Servetus made experiments *in vivo*.

On the whole, Servetus' work appears rather as a product of imagination and fantasy, than as a conquest by scientific methods which were introduced a century later by Harvey.

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UNPUBLISHED AUTOGRAPHS IN THE WELCH MEDICAL LIBRARY

3. An Autograph of Johannes Tackius (1617-1675)

GENEVIEVE MILLER

An example of a course announcement in natural philosophy and medicine at a German University during the middle of the 17th century is seen in the autograph of Johannes Tackius which is preserved in the Schüz Collection of Autographs and Portraits in the Welch Medical Library. (See Fig. 1.)

Tackius was professor of oratory and of medicine at the University of Giessen and the physician to the landgrave of Hesse-Darmstadt. He wrote a number of books, all of which reflected the teachings of the iatrochemist school of thought to which he adhered. He died on August 30, 1675 at the age of 58.[1]

His announcement and its translation follow:

Philosophiae Naturalis et Medicinae Studiosis

Τὸ εὐζῆν καὶ εὐπράττειν

Ad Naturae arcana sanitatisque errores corrigendos denuo vos voco studiosi solertissimi. Praeterita quidem aestate feliciter caeptum erat, at aulica totum nostrum cursum remorabantur negotia. Iam ergo denuo aliquid moliendum est. Adeste ergo frequentes in aedibus meis, qui privata opera mea in naturae rebus indagandis operam collocaturi estis vestram, componam me ad placita vestra sive disputando sive aliud quid agendo, non paenitebit vos temporis, quod physicis impenderitis contemplationibus, faxit Deus, ut feliciter incipiam, felicius quam ante hac progrediar, tandemque vestrum omnium emolumento colophonem addam. Valete Dabatur e Musaeo meo. Sexto Idus Octob. 1652.

ab eo, qui vester totus est,

Joh. Tackius D. M.

[1] *Dictionnaire des Sciences Médicales* (Paris, 1825), Tome VII, p. 294.

To Students of Natural Philosophy and Medicine

My best wishes for good life and good health.

To correct the secrets of Nature and the errors of health again I call you, clever students. Indeed the course had been undertaken successfully last summer, but court business delayed it entirely. Therefore now again something may be started. Therefore be present in my rooms in large numbers, you who are about to arrange your work in investigating the things of nature with my private service. I will place myself at your pleasure, either for disputation or for anything else. The time will not displease you that you will apply to the contemplations of nature. May God grant that I may begin happily, that I may proceed more successfully than before, and may finally come to a successful end for the benefit of all of you.

Farewell Given from my Study. October 10, 1652.

from him, who is wholly yours,

JOHANNES TACKIUS M. D.

Philosophiæ Naturalis
et Medicinæ Studiosis
Τῶ εὐζῆν καὶ εὐπράτειν.

Ad Naturæ arcana, sanitatisq; errores corrigendos
demùm vos Voco Studiosi solertissimi. Præteritâ jam
æstate feliciter cœptum erat, at aulica totum nos
crebrissimè remorabantur negotia. Jam ergo demùm aliqd
moliendum est. Adeste ergò frequenter in ædibus
meis, quò privatâ operâ meâ in naturæ rebus inda-
gandis operam collocaturi estis vestram, componam me
ad placita vestra sive disputando sive aliud qd
agendo, non pœnitebit vos temporis, quòd physicis
impenderitis contemplationibus, faxit Deus, ut feli-
citer incipiam, felicius quam antehac progrediar,
tandemq; vestrum omnium emolumento colophonem addam.
Valete Dabam è Musæo meo. Septo Idus. Octob. ∫ 65 2.

ab eo, q vester toti est.
Joh. Tackius D. m.

Fig. 1.
Autograph of Johannes Tackius (1617-1675).

ANOTHER OSLER LETTER

HUGH STALKER, M. D.
Detroit, Michigan

In my collection of autographed letters, which is to be presented to the Harvard Medical School, is the one quoted below. I often wondered about the misspelling until I came across the following account in W. S. Thayer's "Osler and Other Papers."

Before the Johns Hopkins Medical School opened, the first and third Monday evenings of the month were given to medical meetings. Informal gatherings about the table in the old library, they were attended by most of the staff. Interesting cases from the wards were presented and discussed, and the progress of work in the pathological and bacteriological laboratories was reported. Welch and Osler always came; often Halsted and Kelly, and sometimes Newell Martin from the physiological laboratory across town.

One of the frequent attenders was Doctor V., a charming old man chiefly famed for his rippling laugh, and for having read at an important public meeting a paper in which he had referred repeatedly to "the malarial orgasm."

1 West Franklin Street

October 30, 1897

Dear Norton

Will you examine Mulford boy's blood for malarial *orgasms*. He has enlarged spleen and liver and a history of recurring attacks of fever in Florida.

Sincerely yours,

Wm. Osler

NOTES AND COMMENTS

RE-ECHOES OF SIR WILLIAM OSLER'S "THE FIXED PERIOD"

WILLIAM WHITE, A. M.
The University of Southern California

> The evil that men do lives after them;
> The good is oft interred with their bones.

The lay public, almost wholly ignorant of Sir William Osler's great work in medicine, medical pedagogy, biography and bibliography, seem content to remember him only for remarks he made in "The Fixed Period" regarding the chloroforming of all men over sixty. Two recent references may be cited, one from a national weekly, the other from a metropolitan daily.

Reviewer Mabel S. Ulrich, in her critical notes of Dr. Walter B. Pitkin's *Careers After Forty*, says:

> The results of twelve years of study the author claims to have devoted to this subject have had precisely the opposite effect on this reader from the one he ordained. Dr. Osler's chloroform cure seems hardly less disheartening.[1]

The Santa Barbara correspondent of the Los Angeles *Times* writes:

> SANTA BARBARA, Sept. 4—Hal Garrott, who formerly edited the Carmel Pine Cone in collaboration with Perry Newberry, is here from Monterey Peninsula as a living demonstration that Dr. Osler is not always right. . . . Now, at the age of 60 years, he has begun a career as composer of instrumental music.[2]

This regretable labeling of Osler as the sworn enemy of all men over sixty seems so firmly rooted in the public's mind that little can be done to alter it. Medical students, doctors and other more intelligent readers are now almost the only ones who may chance to pick up a copy of *Aequanimitas With Other Addresses*,

[1] Mabel S. Ulrich, "Dr. Pitkin Prescribes," *The Saturday Review of Literature*, XV, 12, 10 April 1937.
[2] Los Angeles *Times*, Part II-A, p. 12, 5 September 1937.

in which "The Fixed Period" is reprinted.[3] Osler's words, here replaced in their natural setting, have none of the sting and bitterness of a heartless, cold-blooded scientist who has no use whatever for the gentle old folks who mellow our existence, a description of Osler which newspaper men seem so successfully to have foisted upon the lay public.

It is generally known among those familiar with Osler's life and work that "The Fixed Period" was written during a period of emotional stress, when Osler, sentimentally attached to Johns Hopkins medical school, was attempting to assuage his own sorrows and those of his friends at his pending departure. It was a fixed idea with the great physician that men should frequently change the scene of their activities and should, in order to broaden their minds, continue to broaden their associations among members of their chosen profession. He felt that a man, unless he watch himself carefully, cannot avoid aging mentally as well as physically. This was expressed as early as 1892, when, on 4 October, at the opening of the new medical buildings of the University of Minnesota in Minneapolis, he addressed the faculty and students on the subject of "Teacher and Student." With the physical change of silvering hair and lessening elasticity, he said, comes also the mental equivalent, and while the mind may grow clearer and the memory retentive, "the change is seen in a weakened receptivity and in an inability to adapt oneself to an altered intellectual environment."

> It is this loss of mental elasticity which makes men over forty so slow to receive new truths . . . the only safeguard in the teacher against this lamentable condition is to live in, and with the third decade, in company with the younger, more receptive and progressive minds.[4]

This is in the main the same idea he presented thirteen years later in "The Fixed Period," which, misinterpreted, led to his persecution in the world press.

In "The Fixed Period" he recounted the dangers of a professor staying too long in one college, the benefits to the faculties in

[3] William Osler, *Aequanimitas With Other Addresses to Medical Students, Nurses and Practitioners of Medicine*. Second Edition. (Philadelphia: P. Blakiston's Son and Co., Inc., 1930, 475 pp.). Pp. 389-411.

[4] *Ibid.*, "Teacher and Student," pp. 32 f.

changes of personnel, and the advantages of peripatetic life. And he went on to discuss two fixed ideas—the comparative uselessness of men above forty, and the uselessness of men over sixty. He further remarked on

> the incalculable benefit it would be in commercial, political and professional life if, as a matter of course, men stopped work at this age. . . . In that charming novel, "The Fixed Period," Anthony Trollope discusses the practical advantages in modern life of a return to this ancient usage [precipitating sexagenarii from a bridge] and the plot hinges upon the admirable scheme of a college into which at sixty men retired for a year of contemplation before a peaceful departure by chloroform. . . . Whether Trollope's suggestion should be carried out or not I have become a little dubious as my own time is getting so short.[5]

This paragraph the newspapers seized upon, using just enough of it to entirely twist its meaning and lose all the gentle humor it contained.

It was a good opportunity. There had been a mutual antipathy between Osler and the press for some time, arising out of Osler's natural distrust, as a man of science, for journalistic exaggeration. Previously he had remarked:

> Believe nothing that you see in newspapers—they have done more to create dissatisfaction than all other agencies. If you see anything in them that you know is true, begin to doubt it at once.[6]

The feud developed to a culmination after the "Fixed Period" address was delivered, and the nature of the press's treatment of this farewell speech of Osler's may well lead one to believe it was a deliberate attempt at a final blow to the physician. On this score Dr. Cushing, Osler's biographer, makes no charges; but, in several conversations, Dr. Elmer Belt, Los Angeles urologist and Osler enthusiast, has said that the Gentlemen of the Press, somewhat irked by Osler's ill-concealed distrust of them, set out to get him. Certainly they did so with a vengeance, twisting Osler's half-humorous, half-sad remarks into cut and dried judgments. The farewell of the American Press to Dr. William Osler on his depar-

[5] *Ibid.*, "The Fixed Period," pp. 398 f.

[6] Quoted by Harvey Cushing, *Life of Sir William Osler* (Oxford: At the Clarendon Press, 1926. 2 vols.), I, 593 f.

ture for England was a blaze of screaming headlines: OSLER RECOMMENDS CHLOROFORM AT SIXTY, which provoked a mass of vindictive editorial comment all over the world directed against the inhumanity of science in general and Dr. William Osler in particular.

Osler himself seldom alluded afterward to these public notices. Those around him bent every effort toward clearing from his mail the abusive letters which poured in. However, in his preface to the second edition of *Aequanimitas with Other Addresses*, in commenting on the addition to the book of "The Fixed Period," he says:

> To one who had all his life been devoted to old men, it was not a little distressing to be placarded in a world-wide way as their sworn enemy, and to every man over sixty whose spirit I may have thus unwittingly bruised, I tender my heartfelt regrets.

He goes on to say, however, that his opinion has not changed, that the real work of life is done before the fortieth year and that after the sixtieth year it would be "best for the world and best for themselves if men rested from their labours."

There was a certain humor in the situation, but Osler never gave indication, until long afterwards, that he was aware of it. At a subscription dinner held in his honor on 2 May 1908 in New York, just before his departure and a few weeks after delivery of "The Fixed Period," he was presented with a copy from Benjamin Franklin's press (1774) of Logan's translation of "De Senectute." Weir Mitchell made the presentation speech, in which he mentioned "the oldest and the newest authorities on old age." The gift Osler accepted, but he paid no attention to the remarks accompanying it. His only allusion in his address that evening to the whole unfortunate incident was the following sentences:[7]

> I have made mistakes, but they have been mistakes of the head not of the heart. I can truly say, and I take upon myself to witness, that in my sojourn among you:—
>
> I have loved no darkness,
> Sophisticated no truth,
> Nursed no delusion,
> Allowed no fear.

[7] William Osler, *op. cit.*, "L'Envoi," pp. 473 f.

ON THE HISTORICAL INVESTIGATION OF OCCUPATIONAL DISEASES

An Aperçu

GEORGE ROSEN, M. D.

Although the investigation of medical history is being pursued to-day with greater vigor than ever before, there is one field of medicine which has remained comparatively neglected by the medical historians, namely, the occupational diseases. In order to understand this state of affairs certain preliminary considerations are necessary. If one stops for a moment to consider the nature of historical research it is immediately apparent that an analysis of any aspect of this subject must be dual in character; the analysis must consider not only the development of the particular subject matter under consideration but it must also consider the nature of the mental conceptions involved in the methodological approach. Let us first consider the latter factor. It will necessarily be conditioned by the current view of historiography.

The modern conception of history is comparatively recent. From antiquity until well into modern times history concerned itself with affairs of state. As one English historian expressed this viewpoint history was past politics. Neither the masses nor anything concerning them were deemed worthy of interest. With the modern introduction of political and civil equality in the nineteenth century and under the influence of the social sciences with their important discoveries concerning the nature of man and the social organism, the time-honored conception of history had to be revised. Inevitably the political orientation in the study of history gave way to a broader view which took into account, not only war and government, but also industry, commerce, science, technology and everything else that conditions man's social existence. In consequence, towards the close of the nineteenth century the New History, as the all inclusive societal view became known, had given an enormous impetus to the investigation of those aspects of human activity which, precisely because of their assumed inferiority, had received scant attention. Among these social activities, medicine assumed a place as subject for historical research.

The methodological approach was based upon theoretical conceptions conditioned, not only by the development of historiography as outlined above, but also by the development of medicine itself. Despite the fact that medicine in its origins arose in answer to a fundamental social need—the problem of disease—it nevertheless developed to a large extent on an individualistic basis. Disease was dealt with individually; the sick man was treated without much attention being paid to the possible relationship existing between his disease and his background or surroundings. Physicians, accustomed to observe human life, could not fail to recognise that social conditions exerted great influence upon man both in health and disease. Since medicine, however, was developing upon the atomistic basis of the diseased individual as its chief concern, this problem was neglected and remained outside the province of the physician's activities and interests.

The rise of capitalism and the introduction of the Industrial Revolution at the close of the eighteenth century created the masses of the modern industrial proletariat. Forced to crowd into insanitary slums, doomed to starvation by the iron law of wages pontificated by the bourgeois economists and suffering periodically from unemployment, the lot of the industrial workers was a miserable one. Through the initiative of the working class and through the efforts of socially minded individuals the public conscience was aroused to combat these evils. Social questions of the greatest moment—questions of labour legislation, of public aid, and of hygiene and sanitation—agitated the body politic. Legislative and administrative measures calculated to alleviate and remove the worst evils were enacted. The result was that medicine was forced to take notice of and enter into the field of social politics.

From this moment on, the entire viewpoint of medicine was radically altered; now it was concerned not only with the diseased individual but with a more inclusive whole, the diseased social organism. The investigation of the sick man as a social organism demands the investigation of man as conditioned by his environment, his work, his home, his struggles and aspirations. Thus medicine joins hands with sociology and every part of medical knowledge acquires a social content.

Historically industrial medicine is the first product of the social orientation of medicine for industry was the first field of human activity where medicine came into contact with social affairs. The foundations of this branch of medicine were laid in the eighteenth century, but it was only in the nineteenth century, when stimulated by social legislation, that it began to develop rapidly. The investigation of the worker and his diseases naturally led physicians to study the milieu of the laborer, the ultimate goal being the prevention of disease. These investigations supplied medicine with a rich factual knowledge and laid a firm foundation for the development of industrial and social hygiene.

The relation of these considerations to the study of medical history will now be apparent. Problems of medical history may be approached in several ways. The character of these approaches corresponds in a certain sense to the developmental stages in medicine and historiography, which have been considered above. The literature of any medical subject may be investigated with the view in mind of ascertaining the discoveries of earlier authors, which have contributed toward the progress or solution of the particular problem. These positive facts may then be gathered and arranged biographically and chronologically. Essentially this older method of medical historiography is founded upon the same basic viewpoint as the older individualistic medicine. This older method is, however, too superficial and one-sided. Recently as a result of the influence exerted by the New History, new views have begun to prevail in medical historiography. Realising that medicine is but one of the many phases of man's activity as a social being, attempts have been made to explain medical theories and activities as an organic part of the period in which they existed. In other words, using the culture of the particular period as a frame of reference, medicine is shown to exist in a definite relationship to the basic social tendencies of the period. As a result much which had formerly been disregarded, scoffed at and misunderstood, appeared in a new light. Abstruse doctrines, which upon superficial examination had all the semblance of arrant nonsense, were recognised as being the medical reflections of definite cultural currents.

Despite the fact that the cultural conception of medical history

represents a distinct advance over the older theory it does not go far enough in its analysis. Cultural currents—be they philosophical, artistic or political—are no more than the surface manifestations of much more fundamental forces determining the direction of social development.

Society is not a simple arithmetical aggregate of men. The various aspects of human activity are conditioned by the economic structure in which they are embedded and which imposes certain definite relations upon them. The application of this theory to the history of medicine requires the consideration of various factors which had previously remained comparatively unnoticed. It focuses attention on the necessity for investigation of those aspects of social activity which, since they were the concern of the working classes, had not been considered worthy of interest. Technical processes, the daily routine of mine and factory, the conditions under which the worker lives—all are seen to exist in a deep and significant relation to medical thought and practice. A new light is thrown upon the functional relations of medicine in society by considering the physician as an economic unit in a society with a particular class structure. Social conditions, with which medicine is brought into intimate contact undoubtedly influence the development of medical thought and practice and condition the direction which they take. It seems therefore to offer a suitable theoretical method of approach to the complex social and economic problems of medical history. Investigation of these problems offers an opportunity for new discoveries and interpretations in the history of medicine, leading eventually to a profounder understanding of the dynamic processes, which govern the relations between medicine and other social activities.

It now becomes evident that several reasons may be advanced to explain the neglect which the historical study of occupational diseases has experienced. In the first place it is apparent that the study of occupational diseases is one of the youngest branches of medicine and that only within recent times has it come within the province of the practicing physician. The accumulation of a considerable body of factual knowledge was necessary before any historical study could be attempted. Secondly the lack of an appropriate theoretical basis for investigation very definitely retarded the study of this branch of medical history.

Research in the field of occupational medicine needs no special apology. In view of what has been said above, one can readily understand that the results of these investigations will be of great significance, not only in comprehending the development of industrial medicine and hygiene to the condition in which it confronts us to-day, but in a larger sense for the study and understanding of man's social evolution. Historical research on this subject will necessarily concern itself with many factors. The relations of some of these factors to the topic being investigated will not be immediately apparent. A brief glance at the various factors involved in the study of any occupational disease or group of diseases will suffice to demonstrate the numerous ramifications of industrial medicine in society. These factors must of course be located in a definite frame of reference e. g. in a particular time sector. This premise demands as a prerequisite a consideration of the factors which determine the aspect and condition the culture of the period under discussion. According to that theory which appears most promising for the historical investigation of occupational medicine the productive forces at any given time are the basis of the social structure of that time. The methods by which the productive forces are put to work—the mode of production—are conditioned by given factors, the most important being the state of technics during the particular period. Technics include not only material technique,—technology—but also mental technique, i. e. conceptional structures, which arise in answer to definite needs.

The frame of reference, necessary to locate the object of investigation, would then include a general description of the social structure in its economic, technologic, political, religious and cultural aspects. The definition of these elements is indispensable, since the two groups of people with whom the student of the history of occupational medicine is primarily concerned, the workers and the physician are conditioned by these aspects of their society and their relations are to large extent determined by them. Having sketched the basic outlines of the picture, the field of investigation must then be confined to the particular industry under scrutiny. In this narrower field the technical side of the industry must be examined, the working conditions determined and the existential relations of the

worker with respect to aspects of society, such as religion and politics, studied for any bearing they may have on his social status. Once these facts have been established, the medical aspects of the problem must be attacked with the view in mind of ascertaining the diseases described during the period. The extent of medical knowledge pertaining to the nosology, etiology, pathology, clinical aspects and therapeutics of these diseases should be determined. It will be necessary to determine how widespread this knowledge was among practicing physicians and what actually was done to prevent and cure these diseases. These elements will to a large extent be dependent upon the state of medicine at the time. Prophylactic measures on the other hand must be investigated with regard to the state of development of technology and the social status of the worker concerned. The final aspect, which should be investigated, in order to round out the historical picture of industrial disease at any particular time, should be statistical in nature. This information should be concerned primarily with mortality and morbidity, and will naturally depend on the co-existent state of statistics. Since statistics, particularly with reference to disease, are of rather recent origin, any references in earlier periods to this aspect of industrial disease will naturally be vague, meager and rather crudely expressed.

Legislative and administrative measures calculated to eradicate or palliate industrial evils, by the very nature of their purpose, belong in an historical study of occupational diseases. Research will logically concern itself with the origin of these measures, attempting to ascertain the social forces which brought them into being. The part played by the advance of medical knowledge and the role of the workers in obtaining needed industrial reforms will be of special interest.

This sketch has made it sufficiently clear that the time has come for a new appraisal of the history of medicine. It is very likely that the development of research concerning the history of industrial disease will lead the way in this process. Be that as it may, however, the history of occupational medicine will certainly, by explaining their development, contribute to a better understanding of and a deeper insight into many of the pressing problems of our industrial society.

LETTERS OF FIELDING H. GARRISON

Fielding H. Garrison wrote not only many books and articles but also innumerable letters. Many of them are masterpieces. They deal not only with scientific subjects but with all possible questions of literature, art, and, first of all, music. They reveal a highly cultured and very sensitive personality, a restless mind who found it difficult to adjust himself to the post-war world.

The publication of a volume of selected letters of Fielding H. Garrison is being considered. It is felt that such a volume will not only be very welcome to the many friends of Garrison, but will constitute a real contribution to American literature. It will present to a larger public a brilliant writer who so far was known only to a limited group consisting mostly of medical men.

Mr. F. L. Tietsch who was a very intimate friend of Garrison and whose delightful article in the April number of this Bulletin is known to our readers will edit the volume. We felt that no better editor could possibly be found. Mr. Henry Mencken promised his cooperation and placed his very voluminous correspondence with Garrison at the disposal of the editor. Mr. Mencken and I will contribute forewords to the volume.

I wish to invite all the many correspondents of Garrison to send their letters to the Institute of the History of Medicine (1900 E. Monument Street, Baltimore, Md.). The letters will be copied in the Institute and the originals will be returned to their owners without delay. The copies will be preserved in the Institute to serve as materials for a future biography.

While Garrison was very reserved in personal contacts he was exuberant in his letters. His moods and whatever worried him found expression in the letters. Letter-writing was his way to free himself from oppressing thoughts and he not seldom passed harsh judgments on matters and people. It is needless to say that the editor will proceed very carefully in selecting the letters or passages from letters to be published.

HENRY E. SIGERIST.

MEDICO-HISTORICAL ACTIVITIES IN THE UNITED STATES AND CANADA

CANADA

CALGARY, ALBERTA

The Calgary Associate Clinic issues quarterly an *Historical Bulletin* as a supplement to its monthly "Historical Nights." Dr. H. W. Price in a letter of September 30, 1937 writes about the origin of this *Bulletin*:

> "Some six years ago, chiefly under the inspiration of Doctor E. P. Scarlett, the members of the Calgary Associate Clinic began holding monthly evening meetings during the winter at which one or two of the members presented a paper on some aspect of the history of medicine. Usually these papers have been biographical sketches and descriptions of the contributions of the men presented. As each paper has been presented a photograph of the subject, suitably framed, has been hung in the collection in the library here.
>
> While not fostered by the Calgary Medical Society, of which all the members of the Clinic are members, these meetings have developed considerable interest among the members of the profession locally and throughout this part of the province. They are now well attended.
>
> In the spring of 1936 an effort was made to preserve in print a synopsis of each paper presented. Thus the historical bulletin came into being. It is distributed by the Calgary Associate Clinic to the profession in this province."

ILLINOIS

UNIVERSITY OF CHICAGO, CHICAGO

During the last academic year *Dr. Arno B. Luckhardt* gave the following lectures to medical students as integral parts of various courses:

Highlights and Shadows in the Discovery of General Anesthesia
The Reverend Stephen Hales and César J. J. Legallois
Claude Bernard and Carl Ludwig
Thomas Willis and François Magendie
Rise of our Knowledge of the Digestive Glands
Rise of our Knowledge of Reflex Activity
Early Microscopy (Kircher, Swammerdam, Leeuwenhoek, Hooke and Grew)
A Neglected Chapter in the History of Anatomic Illustration and Instruction (Demonstration of 8 ivory Manikins)

The Edwin Smith Papyrus
René Descartes
Charles Bell
Edward Brown-Séquard
Sir Victor Horsley
Hermann v. Helmholtz
The History of Spectacles
Rise of our Knowledge of the Endocrine Glands
Early History of the Thyroid and Parathyroid Glands
Early History of the Pancreas as an Endocrine Gland
Early History of the Hypophysis and Adrenal Glands

NEW YORK

New York University, College of Medicine, New York

The William Welch Society

"The William Welch Society of New York University College of Medicine was organized in 1929 for the 'development and advancement of interest in medical history and culture among medical students.' Since its inception, the society has remained primarily an undergraduate group.

Each month during the college year, student members or prospective members present papers on some historical subject of general interest. To date, we have had discussions on 'Quackery in Medicine,' 'Heinrich Quincke,' 'Jacob Henle,' 'Paul Ehrlich,' 'History of Obstetrical Anesthesia' and 'Public Hygiene in the 15th Century.'

Recently we have begun a series of seminars at which student members survey some one aspect of medical history; and men who have contributed in these fields are asked to discuss their personal impressions of the men under consideration. This year the seminars have been on 'The Development of our Knowledge of Cardiac Irregularities' with Dr. John Wyckoff; 'The History of Digitalis' with Dr. George B. Wallace; and 'The Outstanding Professors of Surgery at Bellevue Hospital' with Dr. Arthur M. Wright.

The third section of our program consists of addresses by two or three invited speakers. Thus far, we have been fortunate in obtaining several outstanding men to address the society. The last meeting of the year was held jointly with the New York University Medical Society, which is a graduate group of the Medical School. One of the guest speakers is asked to address this final meeting. Dr. Foster Kennedy consented to speak last year. His subject was 'The Organic Background of Mind.'"

Julius Schneider.

MARYLAND

JOHNS HOPKINS UNIVERSITY, BALTIMORE

Institute of the History of Medicine

The Institute is glad to be able to acknowledge the following gifts:

An important collection of medical portraits presented by Dr. Lewellys F. Barker.

Ten Osler letters presented from the estate of Miss Adelaide Procter by Mrs. A. M. Lichtenstein.

A bottle of Medicamentum Gratia Probatum and an 18th century fugitive sheet describing the virtues of this remedy were presented by Miss Mary Jacobs.

The following books and pamphlets were received as gifts:

Magni Hippocratis medicorum omnium facile principis, opera omnia quae extant in viii sectiones ex Erotiani mente distributa. Nunc Recens Latina Interpretatione & Annotationibus illustrata; Francofurti, Apud Andreae Wecheli heredes, M. D. XCV.

A Treatise on a Malignant Epidemic, commonly called Spotted Fever; interspersed with Remarks on the Nature of Fever in General, &c. and an Appendix, in which is republished a number of essays written by different authors on this epidemic, with the addition of Original Notes: containing also a few original and selected cases, with clinical remarks. By Elisha North. New York: 1811.

Both were presented by Dr. Simon Flexner.

Dr. Wells, the Discoverer of Anaesthesia. New York, J. A. Gray, 1860. 15 pp.

Presented by Dr. Harry Friedenwald.

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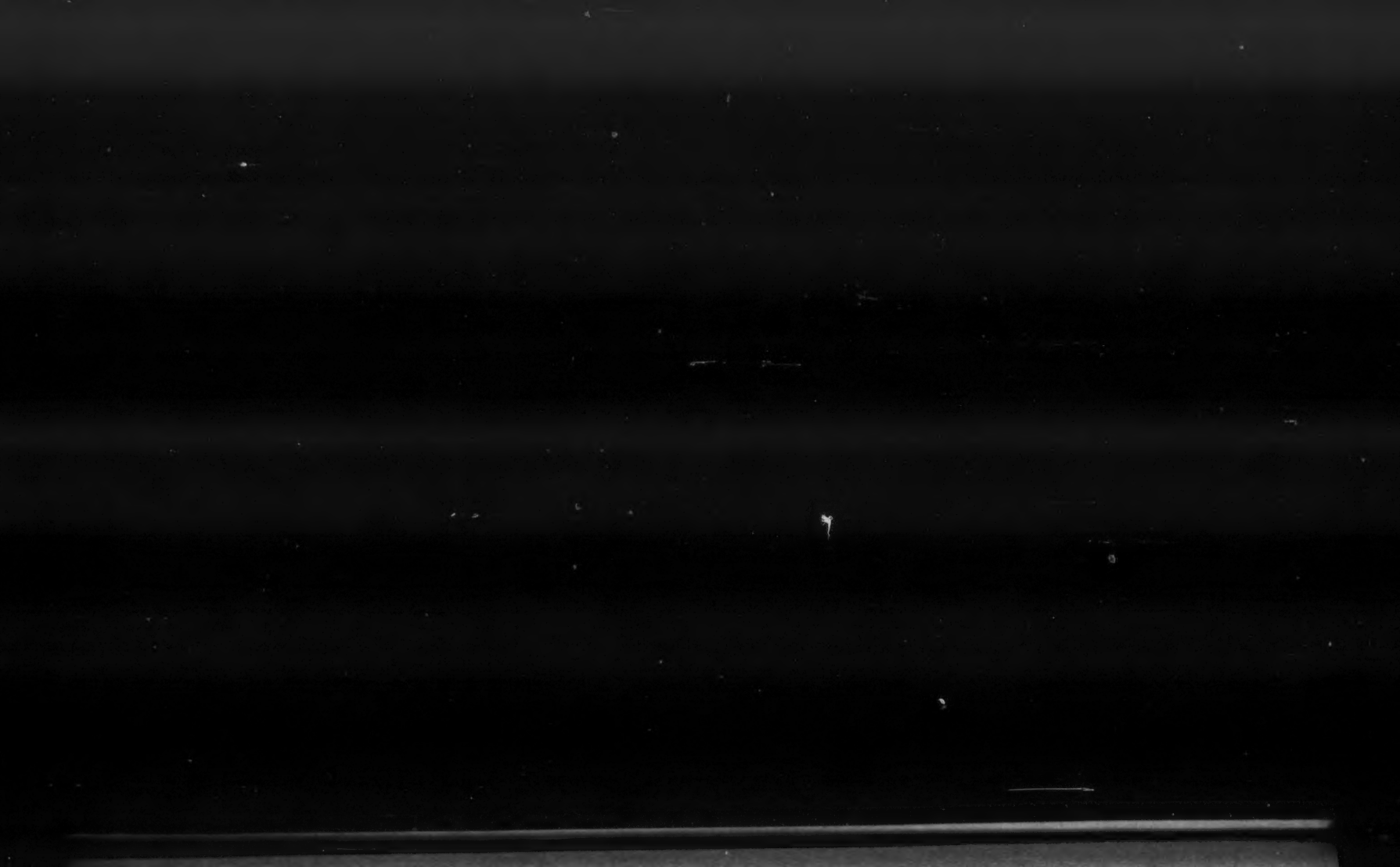
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